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REPORT OF THE SPECIAL COMMITTEE APPOINTED OCTOBER 20, 1886, TO INVESTIGATE THE PROTEST OF POUL LA COUR AGAINST AN AWARD OF THE ELLIOT CRESSON MEDAL TO PATRICK B. DELANY FOR HIS SYNCHRONOUS MULTIPLEX TELEGRAPH SYSTEM.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, May 1, 1887.

At the stated meeting of the FRANKLIN INSTITUTE, held September 15, 1886, a communication was received from Poul La Cour, of Copenhagen, Denmark, dated July 7, 1886, protesting against the award of the Elliot Cresson medal to Patrick B. Delany for his synchronous multiplex telegraph system, on the ground that he had himself invented the system, and that he had taken out patents on it in other countries, but had disposed of his right to do so in the United States, and that his patents ante-dated those granted to Delany.

A copy of this protest was forwarded to Delany, who replied

from Berlin, under date of October 1, 1886, and at the stated meeting of the INSTITUTE, held October 20th, both communications were referred to this special committee with the request to investigate and report upon the merits of La Cour's claim. The members of the committee, although quite familiar with the history of the invention, deemed it best to go over the whole ground carefully to see if any additional facts could be found to change the opinion expressed in the report of the special committee appointed in 1884, upon whose report the award to Delany was made. Inquiries were therefore addressed to the leading telegraph officials of Europe to ascertain what, if anything, was known of La Cour's apparatus prior to the date of the patents issued to Delany and up to the present time. Answers were received from the government directors of telegraphs of Denmark, France and Germany, and their replies together with the communications of Messrs. La Cour and Delany, form a part of the report of this committee. It will be noted that only one of these replies mentions La Cour, although the communication of the French director gives a very interesting history of synchronous multiplex telegraphs in France.

The ground taken by the committee, appointed in 1884, was that La Cour was the inventor of the phonic wheel, upon which, as a basis, Delany founded his system of multiplex telegraphy. The INSTITUTE accepted that view, and, at the suggestion of one of its members, awarded the Scott Legacy Medal to La Cour for that invention. No credit was given La Cour for any device to produce synchronism between two phonic wheels, because the committee were perfectly well aware that not even approximate synchronism had been or could be produced by his apparatus; that after long and unwearied efforts by the most competent experts in America, his method, as described in his patent, was found to be impracticable; also, that La Cour himself, who came to this country, in 1881, to demonstrate the efficiency of his apparatus to the parties to whom he sold his rights in the phonic wheel, failed utterly to produce synchronism by his device.

In the report of the committee of 1884, the fact was emphasized that the essential feature of Delany's system was its practically perfect synchronism, and that without this multiplex telegraphy, by means of the phonic wheel, would be impossible; with it many other methods of telegraphy were possible, including autographic writing.

La Cour saw that the phonic wheel could be made a valuable factor in a multiplex telegraphic system, if he could only produce synchronism between the two wheels. If the specifications in his patents, issued prior to the patents of Delany, do not describe a practicable way to do this, then he has no claim to be the inventor of a synchronous system. It is a general principle, well established in regard to patents, that the description shall be so plain that a person, skilled in the art pertaining thereto, will be able to accomplish the same results as are claimed for the apparatus by the inventor. Now, the proof is abundant that neither by La Cour's system, invented, as he says, in 1881, and which he claims is the same as Delany's, nor by his "improved plan," of October 7, 1882, has any American electrician been able to produce a movement of the two wheels which approximates synchronism so nearly that multiplex telegraphy can be accomplished by it.

The claim is made by La Cour that two circuits over one wire were successfully worked in opposite directions by means of his synchronism, based upon his phonic wheel, between Fredericia and Nyborg, Denmark, a distance of forty miles, in June, 1880. Let us see! In response to an inquiry addressed to the Managing Director of the Danish Government Telegraph, to whom La Cour refers, the following letter was received:

Direction
des Télégraphes d'Etat
de Danemark.

COPENHAGEN, February 9, 1887.

Dear Sir:—In reply to your letter of January 24th, I have the honor to inform you that in the year 1880, during the twenty-four days from the 17th June to 10th July, arrangements were made on the telegraph offices of the Danish Government, at Fredericia and Nyborg, for sending two messages simultaneously over one wire by means of Morse apparatus and separate circuits produced by "phonic wheels," invented by Poul La Cour as synchronous instruments.

I regret that I have not seen the experiments myself, but I am in possession of a diary, held by an officer of the Danish Telegraphs, whereby it is stated that during that time, from the 17th June to the 10th July, 1880, the "phonic wheels," in Fredericia and Nyborg, were working every day from 8 or 9 in the morning to 9 in the afternoon, only with a few short interruptions caused by several events, such as disturbances on the telegraph wire, or mechanical disturbances on the "phonic wheels." For the period from the 22d June to the 10th July, it is expressly remarked that the synchronism was "good."

I am further in possession of official reports, dated 28th June, 1880, from

the head officers of the telegraph offices at Fredericia and Nyborg, after which the apparatuses on the both offices have been working regularly, and messages have been sent since the 17th of June, except two days when the working of the "phonic wheels" was disturbed by mechanical causes. In these reports it is especially stated that on the 27th of June, 1880, were sent *over one wire*; from 9 hours 57 minutes, to 10 hours 19 minutes simultaneously, four messages on each of the two circuits from Fredericia to Nyborg, eighty words on the one and seventy-seven words on the other circuit. From 10 hours 20 minutes, to 10 hours 36 minutes, simultaneously, four messages on each of the two circuits, from Nyborg to Fredericia, sixty-eight words on the one circuit and sixty-three on the other. From 10 hours 48 minutes, to 11 hours 2 minutes, simultaneously, four messages (sixty-eight words) from Nyborg to Fredericia, and four messages (sixty-five words) from Fredericia to Nyborg.

These messages, as well as the paper slips, such as received at the office of Fredericia, are in my hands.

After this, there is no doubt, that in the year 1880, in this country, messages have been sent simultaneously over one wire by means of separate circuits produced by the "phonic wheels," invented by Mr. Poul La Cour. Before that time there was not, nor has there ever since been any telegraph line in this country operated by synchronism as a multiplex system.

With great respect, I remain, very truly yours,

[Signed.] ——— ———*

Director of the Danish Government Telegraphs.

MR. E. ALEX. SCOTT, Chairman of Committee of the FRANKLIN INSTITUTE, Philadelphia.

In the first test, lasting for twenty-one minutes, eighty words were sent on one circuit and seventy-seven on the other circuit, or an average of less than four words per minute on each circuit; or, say, eight words per minute for the whole wire The second test gave about the same average, and the third test a slightly larger one—say, nine words per minute for the whole line.

These experiments were made at the home of La Cour, and presumably under his supervision, and it is fair to assume that the record made was of the best performance of which the system was capable. What do the tests show? Why, most conclusively that there was no synchronism nor even an approximate synchronism. This committee is familiar with the fact that a single wire with one circuit is equal to the transmission, by the Morse system, of from forty-five to forty-eight words per minute, the limit of the ability of expert operators to manipulate the key; and that the same wire, if duplexed, is equal to the transmission of twice as many words,

* Signature illegible. (W.)

and the multiplex system of Delany of six times as many words. It appears, however, that the record of the best performance of La Cour's apparatus shows but nine words per minute for both of the so-called circuits, only one-fifth of what the wire was capable of performing as a single circuit, or one-tenth of what it ought to have accomplished with duplex circuits. It shows that the words actually sent and received were caught by the lucky operator during the brief moments when the instruments had drifted together and before they had separated again; and it may be remarked here, that the more they were out of synchronism the oftener they would drift together.

This is the only reference La Cour has given the committee of a successful working of any system of his where the telegraphing was done supposably by Morse signals. But he says in his communication: "This method being somewhat different from the one now in question, I shall say no more about it," so that we have nothing to do with the only method of his, for which he claims successful results in Morse telegraphy. What we are to look to then, are his later patents, prior to the date of the patents taken out by Delany. What are they?

He says he invented in April, 1881, the system claimed by Delany; that in September, of that year, he sold his patent for the phonic wheel to Mr. Jones, with the right to use any of his later patents; that he took out a patent in England, October 7, 1882, which agreeably to his contract with Jones, he sent to him October 1, 1882. It is this patent marked in La Cour's communication as "Exhibit A," with which La Cour requests the INSTITUTE to compare Delany's synchronism, Delany's descriptions being, as he says, only immaterial modifications of it. A later communication from La Cour to the INSTITUTE, dated March 12, 1887, again refers to this patent as describing a perfectly-working synchronism.

It is a fact, well-known to the committee, that several expert electricians in America, including Delany, Calahan and Yeakle, were unable to produce synchronism by the means described in the patent. Let us see why.

Referring to the drawing accompanying La Cour's patent of October 7, 1882, [*Fig. 1*,] at Station 2, the fork is kept vibrating by a local battery, whose circuit is broken by the fork's vibrations, and the

current is therefore intermittent. The rapidity of the vibrations is decreased by a stronger local battery being substituted for the weaker, the principle being that the wider apart the arms of the fork are drawn asunder, the slower will be its vibrations. La Cour, therefore, directs that the fork at Station 2 be tuned so that its phonic wheel shall turn faster than at Station 1, the fork being operated by the weaker battery. Its phonic wheel, therefore, commences to run faster, and the trailing arm gets ahead of the arm at Station 1, so that a positive current is sent through the line which actuates the polarized relay *R*, and throws the stronger local battery into the circuit. The two wheels are now running apart; they have been running apart all the time, but getting further and

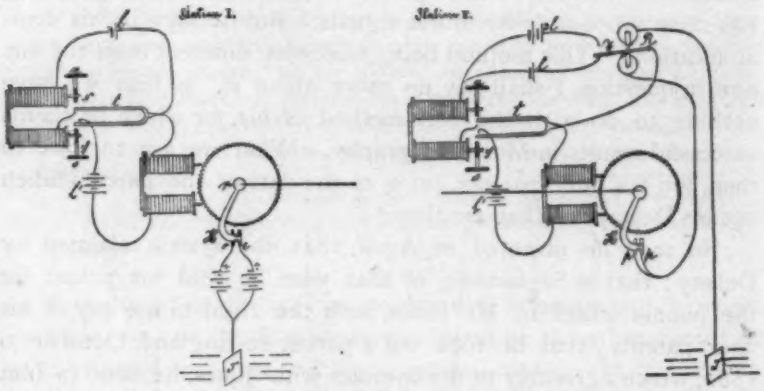

FIG. 1. La Cour's Method.

further apart, because No. 2 has been constantly gaining. No. 2 has run faster, because its local battery was too weak; before, however, it commences to feel the effect of the stronger local battery, which will tend to check its speed, it is left without any battery at all, while the armature of the correcting relay passes from one stop to the other; this gives it a still greater speed and puts the two wheels still further apart. When the stronger battery is substituted, this process is reversed; No. 2 commences to run slower, and as the wheels are now widely apart, the battery must be strong enough to make it run slower than No. 1. No. 1 finally overtakes it, and for a brief moment the two wheels are synchronous. But as No. 2 is now running slower than No. 1, it falls behind it, and the two wheels drift apart again until a negative

current is sent through the line, which throws the armature of the polarized relay to the other stop and puts on the weaker battery. Thus, as La Cour states in his patent: "The relay will cause the wheel at No. 2 to move on the average just as quickly as the wheel at No. 1."

It is hardly necessary to say that this is *not* synchronism. The wheels are *compelled* to be out of synchronism all the time. It is not even close enough to be approximate synchronism, and it would be impossible to use such a system even for duplexing a line, to say nothing of creating a greater number of circuits.

Delany having found La Cour's method impracticable, abandoned it, and set about constructing a new system by which each wheel kept the other from running irregularly, not by bringing it back after it had gotten out of synchronism, but by preventing it from getting out, the slightest tendency to go out being immediately checked. This was done by the introduction of resistances into the fork circuit, which, being instantaneously introduced, operated immediately. This was a radical change in three particulars: (1), in the mutual correction by the two wheels of each other's movements; (2), in the application of the correction before the wheels were out of synchronism, and (3), by the instantaneous introduction of *resistances* into the circuit.

The especial object of La Cour's letter of March 12, 1887, was to call attention to the fact that G. A. Cassagnes, a French engineer, successfully operated his synchronous autographic system, which he calls steno-telegraphy, by means of his (La Cour's) synchronism as described in La Cour's patent of October 7, 1882. The Committee found in the Memorial Library of the INSTITUTE, a copy of a pamphlet issued by Cassagnes in 1886, descriptive of his system of steno-telegraphy, which states that the system is based upon the synchronism produced by La Cour's phonic wheel. Drawings of the apparatus are given in the pamphlet which show *not* La Cour's system of two batteries of different strength, but Delany's method of instantaneous introduction of resistances. (See *Fig. 2*, page 88,)

It is unnecessary to enumerate the many minor details required to produce the results effected by Delany. Suffice it to say that, having perfect synchronism between the two wheels when standing side by side, that alone, was not sufficient to produce multiplex

telegraphy, as Delany found to his cost when he put his apparatus to a practical test on a telegraph line. A new problem awaited him. After the passage of every current of electricity over the line, the static charge of the line discharged itself through the succeeding segment, producing an effect similar to that produced by the battery current. Signals, therefore, sent on one segment were received on two or more.

To dispose of, or neutralize, or utilize this current was the task which Delany had now to undertake. La Cour ignores it in his

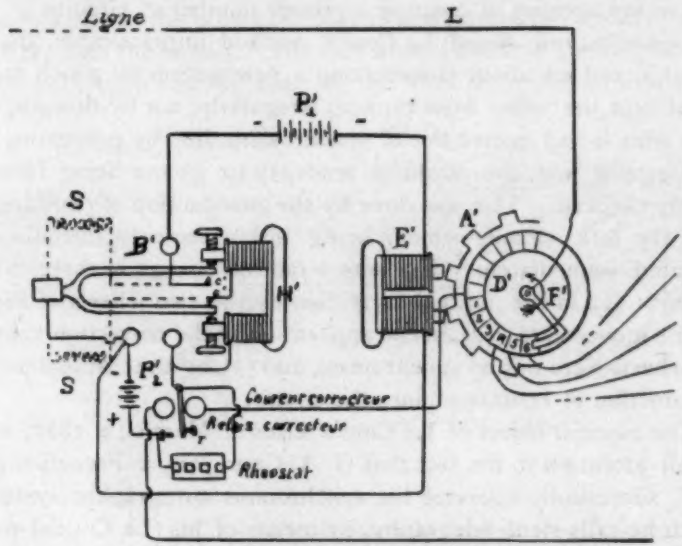

FIG. 2. From Cassagnes's Pamphlet.

patents: he seems never to have gotten far enough with his investigations in multiplex telegraphy to have encountered this difficulty, and yet it is one of the most perplexing problems with which the telegraph engineer has to deal.

The INSTITUTE is familiar with the method by which Delany disposes of this static charge by the introduction of alternate ground segments between the line segments, and it is unnecessary for the purposes of this report to show how the Delany system has reached its present condition, surpassing as it does all known systems of Multiplex Telegraphy. It is sufficient to know that without synchronism, which La Cour did not have, and without discharging, or in some way neutralizing, the static charge of theline,

which La Cour did not attempt to do, there is no such thing as multiplex telegraphy by means of the phonic wheel.

The Committee beg leave to say that in their opinion, full justice was done to La Cour in the award to him of the Scott Legacy Medal and Premium for the invention of the phonic wheel; but that to Delany belongs the credit of inventing and perfecting a highly successful synchronous system of multiplex telegraphy based upon the phonic wheel.

The Committee therefore respectfully report, that they find no substantial reason for revoking the award of the Elliot Cresson Medal to Patrick B. Delany, already approved by the INSTITUTE.

[SIGNED.] E. ALEX. SCOTT, *Chairman.*
W. W. GRISCOM,
EDWIN J. HOUSTON,
A. E. OUTERBRIDGE, JR.,
WM. H. WAHL.

APPENDIX.

Contents.

(1.) Protest of Poul La Cour with Patent Specification of Frederick Wolff.
(2.) Reply of Patrick B. Delany to La Cour's Protest.
(3.) Letter from the Director of Danish Government Telegraphs.
(4.) Letter (and translation) from the Minister of the French Post Office and Telegraphs.
(5.) Letter (and translation) from the Imperial German Postal Department.
(6.) Letter from Poul La Cour, accompanied by documents relating to M. Cassagnes's system of steno-telegraphy.
(7.) Letter of the Secretary of the FRANKLIN INSTITUTE to Poul La Cour.

APPENDIX 1.

(*La Cour.*)

COPENHAGEN, July 7, 1886.

TO THE FRANKLIN INSTITUTE, Philadelphia:

The most honorable INSTITUTE has shown me the courtesy to recommend the city of Philadelphia to assign me to the John Scott Medal with the attached premium for my invention of the phonic wheel.

In cordially thanking the INSTITUTE for the attention thus created for my invention, I beg leave to make the honorable INSTITUTE acquainted with, that the synchronic system which Mr. Patrick Delany has placed before the INSTITUTE as his system, and which the committee of the electrical section has spoken so favorably about, as appears from the JOURNAL OF THE FRANKLIN INSTITUTE, April, 1886, is invented by me. But having, as will be

shown below, renounced upon the right of a United States patent, the honorable committee could not be aware of, that I had the patents in other countries long before Mr. Delany took out a patent in the United States.

To illustrate the matter perfectly, it will be necessary to discern between the three things:

(1.) The invention of the phonic wheel.

(2.) The invention of the absolute synchronic movements by aid of the phonic wheel.

(3.) The applications of the synchronism for telegraphy of different kinds.

Mr. Delany does not attempt to claim the invention of the phonic wheel, and the honorable INSTITUTE has further confirmed this by the recommendation to the John Scott Medal, as is also expressed in the aforesaid report of committee (JOURNAL OF THE FRANKLIN INSTITUTE, April, 1886, page 313).

Regarding items (2) and (3), the committee says that item (2) is the main point.

It runs as follows, page 318: "The committee recognizes the fact that the practically perfect synchronism attained by this invention is a most important advance in the art of telegraphy. With synchronism many other things are possible, and it seems quite probable that its uses will not long be confined to the two systems of telegraphy herein described or even to telegraphy alone." The only question is then about the priority of the invention of the synchronism, and to that effect I beg to substantiate.

(1.) That a perfectly accurate automatic synchronism of a somewhat different system, based upon the phonic wheel, existed several years before Mr. Delany engaged himself about it.

(2.) That actually the same synchronism described in the report of the committee was invented and published by me, prior to Mr. Delany bringing his invention before the public, and

(3.) That Mr. Delany has not invented this synchronism himself, but has had immediate admittance to a descriptive information from me about the method.

I take it for granted that Mr. Delany's first publication is a United States Patent, taken by Calahan and Delany, dated 17th July, 1883, as this patent refers only to my patent for the phonic wheel formerly taken, and not at all to any prior works by Calahan and Delany.

I have therefore to prove that my inventions and publications are of earlier dates.

In June, 1880, was in use for some time incessively from morning till night, a synchronism based upon the phonic wheel, between Fredericia and Nyborg. The Managing Director of the Danish Government Telegraph, has given a testimony to show that this synchronism worked perfectly accurate and reliable, and was easy to manage. But this method being somewhat different from the one now in question, I shall say no more about it. It shows, however, that the inventions since 1880, can only be termed "improvements," not in accuracy but by regulating the synchronism by the telegraph lines in the shortest space of time possible, and thus the line can transmit a greater portion of telegraphy.

In April, 1881, I invented the system of synchronism which Mr. Delany has published as his, but I did not take out patents at once. After having sold, in September, my United States patent for the phonic wheel to a Mr. Jones, to whom I promised to communicate all further improvements with the right of taking out patents in the United States, I took a patent in England, 7th October, 1882. Description of this I sent to Mr. Jones, as promised, on 1st October, 1882, and a letter from him dated 8th November, 1882, proves that he has received it, as also another letter from Mr. Jones, in which he says: "Delany and Calahan both are working in harmony with my interests here."

Mr. Delany, I suggest, must have had immediate access to my description, and in the course of 1883 (specially 9th October, 1883), he took out a number of patents of different modifications of the same invention.

I enclose my patent for England, dated 7th October, 1882 (see Exhibit A), and the most honorable INSTITUTE will, by closer examination, find that Mr. Delany's descriptions are only immaterial modifications thereof, without any theoretical or practical importance.

My synchronism is maintained by letting the time of oscillation of the tuning fork vary, as said in my description to patent, viz.: The time of oscillation can be lengthened by:

(1.) Moving the attractive poles nearer to the branches of the tuning fork, or

(2.) By letting a stronger current act upon the tuning fork to keep it in motion.

The first alternative is specially adapted for pan-telegraphy, as the receiving telegraph clerk must regulate the rapidity himself in order to get exact copies.

The second alternative, on the contrary, makes the synchronism automatic, as a corrective current acts upon relays, and thereby letting a stronger or weaker local current act upon the tuning fork according to its oscillating too quick or too slow.

Every electrician knows that a current can be make stronger or weaker in different ways.

It is quite indifferent whether this be done by the relays letting in different resistances in the current or a different number of batteries.

Mr. Delany has, however, not been successful in his choice, as it is not necessary to make use of six contact pieces for each rotation of the wheel, only for preserving the synchronism.

In Denmark, Paris, Vienna, etc., where I have done the arrangements, two, even very narrow pieces, have been quite sufficient for the synchronism, and the rest have been available for telegraphy.

Further, it is of little or no importance to make special arrangements for proving when the synchronism is obtained, as it puts itself right and *cannot unorder* itself, as was evident by my arrangements at the Exhibition, Vienna, August–October, 1883, and is further proved by the practical use of my synchronism by Mr. Cassagnes's steno-telegraph between Paris–Marseilles.

Mr. Delany's description adds nothing particularly new to what is said in my patent of 7th October, 1882, but only serves to complicate the system which is perfectly reliable in the simplified form.

Always ready to give any desired information, I solicit that the most honorable INSTITUTE will give this publicity. I remain, gentlemen,

Yours respectfully, POUL LA COUR

I have examined the documents referred to by *stars* in this letter, and find they have been correctly cited.

B. B. ANDERSON, *U. S. Minister.*

Copenhagen, Denmark, July 8, 1886.

(*La Cour.*)

EXHIBIT A.

Specification of Frederick Wolff.

[A. D. 1882, 7th October, No. 4,779.]

OBTAINING SYNCHRONOUS MOVEMENTS.

LETTERS PATENT to Frederick Wolff, of the International Patent Office, Copenhagen, Denmark, Patent Agent for an invention of IMPROVEMENTS in OBTAINING SYNCHRONOUS MOVEMENTS AND APPARATUS THEREFOR. A communication to him by Poul La Cour, of Askovhus, Vejen Station, in the Kingdom of Denmark.

PROVISIONAL SPECIFICATION left by the said Frederick Wolff at the office of the Commissioners of Patents on the 7th October, 1882.

FREDERICK WOLFF, of the International Patent Office, Copenhagen, Denmark, Patent Agent, "improvements in obtaining synchronous movements and apparatus therefor" (a communication from Poul La Cour, of Askovhus, Vejen Station, in the Kingdom of Denmark).

An apparatus for obtaining synchronous movement according to this invention, consists principally of a vibrating body, such as a tuning fork, permanently vibrating or oscillating by automatic electric intermittent action, and thereby transmitting an intermittent or undulating electric current, and of a wheel subject to oscillation or vibration under the influence of the said current. The rotation of this wheel, under these circumstances, being exceedingly regular, the current is applicable to various purposes, such, for example, as to establish synchronous movements at two or more stations connected by a telegraphic line.

The more the oscillating or vibrating body (hereinafter denominated a tuning fork), is attracted, the longer is the period or rate of its oscillation and the slower does the wheel consequently move.

Thus the time of oscillation can be lengthened, for instance by—

(1.) Moving the attracting poles nearer to the branches of the tuning fork, or

(2.) By letting a stronger current act upon the tuning fork to keep it in motion.

The former can be done simply by applying through the poles *N* and *S* of the electro-magnet two screws of iron forming polar extensions.

When these are screwed nearer to the branches of the fork or farther therefrom, the time of vibration or oscillation of the fork will accordingly be

somewhat prolonged or shortened, and the wheel will consequently turn somewhat slower or somewhat quicker.

This method of regulation can be adapted to apparatus at various stations, connected by a telegraphic line, when it is easy to tell if the movements are synchronous.

If the synchronous movements are used, for instance, to produce pan-telegraphy, it can directly be observed at the receiving station if the writing or the drawing stands correctly, or, still better, if a vertical line on the original for instance, along the border of the same, appears as a vertical line at the receiving station.

If the line is reproduced in an oblique position, bending to the one or to the other side, it proves that the revolution of the phonic wheel is too quick or too slow.

The screws forming the polar extensions can then be regulated, until the copy of the vertical line also becomes vertical.

By the latter of the above mentioned methods for altering the time of oscillation of the fork, namely, by varying the strength of the current, which causes the fork to oscillate, one can obtain absolute synchronous movements in the following manner.

At Station No. 1, [see *Fig. 1*, page 86,] the fork is kept in automatic movement by a local battery.

The circuit of a second local battery is thereby intermittently completed, and the phonic wheel can thus be kept in uniform rotation, one tooth passing at each oscillation or vibration of the fork. At Station No. 2, the fork is in a similar way kept in automatic movement by means of the local battery.

Thereby the current in a second local battery becomes intermittent and the phonic wheel has a corresponding speed of rotation.

At Station No. 2, there is also a third local battery, stronger than the first, and capable of being thrown into circuit in place of the first by the tongue of a polarized relay.

The fork is now tuned at Station No. 2, by means of the polar extensions, so that the phonic wheel at No. 2, turns quicker than at No. 1, when the fork at No. 2 is operated upon by the first local battery, whilst the wheel at No. 2, turns slower than the wheel at No. 1, when the fork at No. 2 is operated upon by the third local battery.

To each of the wheels is attached an arm which is in electric connection with the telegraphic line.

The arm at Station No. 1, touches at each revolution two contact pieces, which are in connection respectively with the negative and positive poles of two batteries, the other poles of which are conducted to the earth.

Consequently there is produced at each revolution a negative and positive undulation of short duration. At Station No. 2, there are two corresponding contact pieces, from which a circuit passes through the convolutions of the relay to the earth.

On putting both wheels in motion and placing the tongue of the relay in such a position that the fork at No. 2 is operated by the first battery, the wheel at Station No. 2 will turn quicker than the wheel at No. 1.

Thereby the arm at No. 2 will gradually overrun the arm at No. 1, so that the former touches its contact pieces at the same moment as the latter touches its positive contact piece.

The tongue of the relay stands still, however, until the arm at No. 2, after some more revolutions, touches its terminal in the very same moment that the arm at No. 1 touches the negative terminal.

The negative current wave will throw the tongue of the relay against the other contact, and thus the third battery is substituted for the first, whereby the wheel at No. 2 turns slower. After some revolutions, the arm at Station No. 2 will again touch the contact at the same moment the positive wave from No. 1 arrives to move the relay tongue, whereupon the first battery is again replaced, and so on.

Thus the relay will cause the wheel at No. 2 to move on the average just as quickly as the wheel at No. 1, and for that purpose the telegraphic line is only in use during the short time the arms at the two stations are passing respectively over the contact pieces.

During the rest of the time, the telegraphic line can be used for various kinds of telegraphy, the arms being available during the remainder of their motion to send and receive different currents from No. 1 to No. 2, or from No. 2 to No. 1.

Or, there can during some fractional parts of the revolution be sent currents in one direction, and during others in the opposite direction.

Thus the synchronous movements can be utilized in different kinds of telegraphy.

SPECIFICATION in pursuance of the conditions of the Letters-Patent filed by the said Frederick Wolff in the Great Seal Patent Office, on the 3d April, 1883.

FREDERICK WOLFF, of the International Patent Office, Copenhagen, Denmark, Patent Agent. IMPROVEMENTS IN OBTAINING SYNCHRONOUS MOVEMENTS AND APPARATUS THEREFOR (a communication to me by Poul La Cour, of Askovhus, Vejen Station, in the Kingdom of Denmark.)

An apparatus for obtaining synchronous movements, according to this invention, consists principally of a vibrating body, such as a tuning fork permanently vibrating or oscillating by automatic electric intermittent action, and thereby transmitting an intermittent or undulating electric current, and of a wheel subject to oscillation or vibration under the influence of said current. The rotation of this wheel, under these circumstances, being exceedingly regular, the current is applicable to various purposes, such, for example, as to establish synchronous movements at two or more stations connected by a telegraph line. The more the oscillating or vibrating body (hereinafter denominated a tuning fork) is attracted, the longer is the period or rate of its oscillation, and the slower does the wheel consequently move. Thus the time of oscillation can be lengthened, for instance, by (1), moving the attracting poles nearer to the branches of the tuning fork, or (2), by letting a stronger current act upon the tuning fork to keep it in motion.

The former can be done simply by applying through the poles *N* and *S* of the electro-magnet two screws of iron forming polar extensions. When these are screwed nearer to the branches of the fork, or farther therefrom, the time of vibration or oscillation of the fork will accordingly be either a somewhat prolonged or shortened, and the wheel will consequently turn somewhat slower or somewhat quicker. This method of regulation can be adapted to apparatus at various stations, connected by a telegraphic line, when it is easy to tell if the movements are synchronous. If the synchronous movements are used, for instance, to produce pan-telegraphy, it can directly be observed at the receiving stations if the writing or the drawing stands correctly, or, still better, if a vertical line on the original, for instance, along the border of the same appears as a vertical line at the receiving station. If the line is reproduced in an oblique position, bending to the one or to the other side, it proves that the revolution of the phonic wheel is too quick or too slow. The screws forming the polar extension can then be regulated, until the copy of the vertical line also becomes vertical.

By the latter of the above mentioned methods for altering the time of oscillation of the fork, namely, by varying the strength of the current, which causes the fork to oscillate, one can obtain absolute synchronous movements in the following manner.

Referring to the accompanying sheet of drawings, at Station No. 1, the fork *t* is kept in automatic movement by a local battery 1. The circuit of a second local battery 1^1 is thereby intermittently completed and the phonic wheel *r* can thus be kept in uniform rotation, one tooth passing at each oscillation or vibration of the fork. At Station No. 2, the fork *t* is in a similar way kept in automatic movement by means of the local battery 1.

Thereby the current in a second local battery 1^1 becomes intermittent and the phonic wheel *r* has a corresponding speed of rotation. At Station No. 2 there is also a third local battery 1^2 stronger than the first 1^1 and capable of being thrown into circuit in place of the first by the tongue of a polarized relay *R*. The fork *t* is now turned at Station No. 2 by means of the polar extensions *N* and *S* so that the phonic wheel at No. 2 turns quicker than at No. 1, when the fork at No. 2 is operated upon by the first local battery 1, whilst the wheel at No. 2 turns slower than the wheel at No. 1 when the fork at No. 2 is operated upon by the third local battery 1^2. To each of the wheels is attached an arm *H* and *H* which is in electric connection with the telegraphic line. The arm *H* at Station No. 1 touches at each revolution two contact pieces *e* and *o* which are in connection respectively with the negative and positive poles of two batteries, the other poles of which are conducted to the earth *T*. Consequently there is produced at each revolution a negative and positive undulation of short duration. At Station No. 2 there are two corresponding contact pieces *p* and *p*, from which a circuit passes through the convolutions of the relay *R* will cause the wheel at No. 2 to move on the average just as quickly as the wheel at No. 1, and for that purpose the telegraphic line is only in use during the short time the arms *H* and *H* at the two stations are passing respectively over the contact pieces *e o* and *p p*. During the rest of the time the telegraphic line can be used for various kinds

of telegraphy, the arms *H* being available during the remainder of their motion to send and receive different currents from No. 1 to No. 2, or from No. 2 to No. 1.

Or there can during some fractional parts of the revolution be sent currents in one direction, and during others in the opposite direction.

Thus the synchronous movements can be utilized in different kinds of telegraphy.

Having described the nature of the said invention and the manner of carrying it into practical effect as communicated to me as aforesaid, what I claim is:

(1.) Utilizing the variable attraction between an electro-magnet and a permanently vibrating or oscillating body, such as a tuning fork for the purpose of altering the period or rate of vibration or oscillation of the said body the greater attraction giving the slower rate of vibration or oscillation substantially as described.

(2.) Varying the velocity of rotation of the phonic wheel by altering the distance between the polar extensions of the electro-magnets and the branches of the fork; or by varying the strength of the current that operates the tuning fork *substantially as described*.

(3.) Regulating the velocity of rotation of the phonic wheel at the receiving station, by means of a current from the transmitting station acting through a relay, whereby the strength of current operating the tuning fork at the receiving station, is changed as required to render synchronous the rotation of the phonic wheel at the two stations substantially as described.

In Witness Whereof, I, the said Frederick Wolff, have hereunto set my hand and seal this twenty-seventh day of March, in the year of our Lord one thousand eight hundred and eighty-three.

FREDERICK WOLFF. [L. S.]

APPENDIX 2.

(*Delany.*)

103 *A* Potsdamer Strasse,
BERLIN, Prussia, October 1, 1886.

To the FRANKLIN INSTITUTE:—Through the courtesy of your honorable Secretary I have been furnished with a copy of a communication, addressed to the FRANKLIN INSTITUTE, by Mr. Poul La Cour, disputing my claim to the invention of practical synchronism and synchronous multiplex telegraphy, for which invention the FRANKLIN INSTITUTE has highly honored me by awarding me the Scott Legacy and Elliot Cresson Medals. I take the earliest opportunity to reply briefly to Mr. La Cour's communication, and at the same time to assure your honorable body that if any further explanation or information is desired to prove the correctness of my statements, I hold myself in readiness to furnish the same, or to prove by actual demonstration on my return to America that the FRANKLIN INSTITUTE has not been mistaken in its generous award.

Mr. La Cour's American patent for "Improvements in Isochronous and Synchronous Movements for Telegraphic and other Lines" bears date May 7, 1878. Its history, so far as I have been able to ascertain, is that it was first placed in the hands of a Mr. Yeakle, of Baltimore, a practical telegrapher and electrician, who was unable to make it work. Later it was submitted to the Western Union Company's electricians in New York, Mr. D'Infreville and others, who pronounced it impracticable. Still later, it was submitted to Messrs. G. S. Mott and James G. Smith, well-known electricians, practical telegraphers and inventors, who also pronounced it impracticable. It was then given in charge of Mr. E. A. Calahan, of New York, also a well-known practical telegrapher and inventor, who experimented with it for nearly a year, with no better success than his predecessors. Some time in 1881 the parties interested in the patent brought Poul La Cour, the inventor himself, from Denmark to New York, for the purpose of obtaining from his patent some of the results claimed for it, but so far as I have been able to learn, Mr. La Cour failed utterly in his mission, and returned home without having obtained synchronism.

In the winter of 1881–82, my attention was first called to the invention by Mr. Calahan, at whose urgent request I became interested in an effort to obtain synchronism from it, the understanding being that as soon as synchronism was obtained, parties interested would purchase the patent from La Cour. After several months constant endeavor, I came to the conclusion that synchronism in the manner set forth was impossible, and in the light of subsequent experience that conclusion has been fully sustained and proven. In the summer of 1882, experimentation led me to think that synchronous telegraphy could be accomplished by using *two* line wires, one for vibrating the forks in unison, the other for telegraphic purposes. Upon *this* representation to the parties interested, the patent was bought from Mr. La Cour.

In the fall of 1882, Mr. La Cour submitted a plan for maintaining synchronism, in all respects the same as that set forth in his English Patent, October 7, 1882, of which he has sent you a copy. I have the original manuscript describing that plan. Mr. Calahan and myself carefully followed the instructions laid down, and although several days were spent in trying, we were unable to obtain synchronism for one minute. We had, by constant changing of adjustments, obtained accidentally approximate synchronism for a few minutes by the original plan of La Cour during a period of nearly six months, but failed utterly to obtain any encouragement from the new plan.

Mr. La Cour states that in June, 1880, he had constant synchronism between Fredericia and Nyborg, and that the Danish Director of Telegraphs has testified to the fact. This was nearly a year before the invention of his improved plan, which he himself states was invented in April, 1881.

Now, with all due respect to the civilities of life, I must simply state that such a thing was, is, and always will be absolutely impossible. If the INSTITUTE will examine La Cour's United States Patent 203,423, they cannot fail to find that synchronism under such conditions (and they are the same as

used by La Cour between Fredericia and Nyborg) is a physical impossibility. The conditions were, briefly, these:

At one station a fork made and broke automatically its own circuit through a local battery at a rate of about ninety times per second. This fork rotated a phonic wheel, the trailing arm of which made contact with numerous segments placed in the path of its rotation. With six of these segments, a main battery was connected. At the distant station was placed another fork and phonic wheel. This fork was not vibrated by a local battery, but by the six impulses which came over the line at each rotation of the trailer at the first station, numbering in all about thirteen per second, *provided the second phonic wheel was first in synchronism* with the first phonic wheel; otherwise the impulses coming over the line could not be delivered to the fork magnet. With these eighteen impulses over a line, the second fork was expected to vibrate in synchronism with the fork at the first station, which made for itself automatically ninety vibrations per second. Synchronism was first to be obtained by spinning the wheel by hand in order to get the fork at the second station to vibrate, and when once in vibration, this fork, with eighteen impulses per second, was to vibrate synchronously with the other, and without any correcting application whatever; and yet Mr. La Cour tells the FRANKLIN INSTITUTE that by this method he had for some time, incessantly from morning till night, synchronism between Fredericia and Nyborg, and that the managing Director of the Danish Government Telegraph has given a testimony to show that this synchronism works perfectly accurate and reliable, and was easy to manage.

Four years trial in America by competent and skilful electricians and inventors, including Mr. La Cour himself, failed to bring about in the slightest degree any such results. Even though the fork at the second station had been vibrated automatically by a local battery in the same manner as the fork at the first station, synchronism without correcting impulses would have been impossible. I have tried it thousands of times with instruments side by side.

The INSTITUTE can form its own opinion as to the correctness of Mr. La Cour's statement, that he had perfect synchronism "from morning till night."

Now, to return to Mr. La Cour's improved plan as communicated to me through Mr. Jones, and as exhibited by him (La Cour), in Denmark, Paris, Vienna, etc., and which he charges is substantially the same as used by me, I beg to state that this plan is not only not synchronous, but absolutely *anti-synchronous*, for, as the committee will find upon examination, and as I can prove by demonstration, should instruments be brought to synchronism, accidentally or coincidentally, the means provided by Mr. La Cour for regulating them in synchronism would drive them out of synchronism. Reference to his description and drawings will show that the trailer *H* at Station 2, is expected to touch *two* contacts *p p*, while the trailer at Station 1 is on one of the contacts *o e*. And no matter which position the armature of the polarized relay is in, the fork and wheel *must be going either too fast or too slow*. They are not allowed to remain synchronous. Again, supposing the armature *T* of the polarized relay is in the position shown in the drawing, the fork is then

vibrating too fast, a reverse current throws the armature over to its other stop to make it go slow, but while the armature is passing from the fast to the slow battery, the fork circuit being *entirely broken*, the rate of vibration of the fork is enormously increased, and the instruments driven away from even approximate synchronism. Therefore, when the distributor at Station 2 requires a retarding correction, it must first be accelerated far beyond the rate which called for retardation.

It must also be understood that the most delicate balance will not prevent the instruments from constant variation. If the change from the strong to the weaker battery is sufficient to *accelerate* the speed of the second instrument against its natural tendency to go slow at the time, this acceleration being *constant*, will also be sufficient to cause the instrument to gain sufficiently in one revolution, to entirely miss the segment for a retarding impulse on the next revolution, and *vice-versa*.

Assuming that a line, say of fifty miles or upwards, connects the two instruments, if a correcting impulse for slowing is needed, the trailer at Station 1 must be on battery segment *e* when the trailer at Station 2 is on the left hand contact *p*, then on account of retardation of the impulse in the line, the impulse will not arrive at Station 2 until the trailer at that station has passed off of the left-hand segment *p*, and no correction will be applied.

Again, even if it were admitted that all else worked as Mr. La Cour claims it will, it must be assumed that his correcting segments to which the batteries are connected, are as broad as the other segments used for telegraph signals (for otherwise the correcting impulses would not charge the line sufficiently to be felt at the distant end) then this being the case, it follows that the trailer at Station 1 must be, say, half way on the battery segment before the line can be charged sufficiently to change the position of the armature of the polarized relay, and, consequently, the variation of the trailers would be from the middle of one segment to the middle of the other, or equal to two segments, for the spaces between the segments must be included, and each space must be equal to the width of each segment to prevent interference between the segments forming the circuits. Therefore, the instruments would be out of synchronism two-thirds of the time. The charging point is indicated by

FIG. 3.

the dots on the segments. Mr. La Cour makes no provision for static discharge, "tailing," or retardation of the current. The quality of all his corrections must be the same; a slight variation cannot be rectified, nor is the

correction in any way proportioned to the discrepancy in synchronism. I do not deem it necessary to go into comparisons between Mr. La Cour's attempts and my synchronism, believing, as I do, that the FRANKLIN INSTITUTE fully appreciated the difference when it conferred its honors.

Mr. La Cour's phonic wheel patents have been abandoned in every country in Europe, and undoubtedly would have been forgotten in America, had it not been for my synchronism. His efforts for synchronism have been tried by various Government telegraph administrations without any success whatever. In a recent interview with Mr. La Cour, in Berlin, he admitted to me that he had no telegraph system, and in the presence of Herr Dr. Prof. Zetzsche, of the *Electro-Technische Zeitschrift*, he positively declined to discuss synchronism as a relative term, or the degree of synchronism claimed by him. I challenged his ability to obtain two circuits over a single wire, but he declined it. It is not necessary for me to remind the FRANKLIN INSTITUTE that synchronism at best is but a relative term; that it is all a question of degree. The Highes, Meyer and Baudot systems are all synchronous

FIG. 4.

within certain limits. Two trailers, touching corresponding sectors of these circles, might be called synchronous. But synchronism on segments one millimetre in width, in a circle five inches in diameter, with trailers revolving at the rate of three times per second, is quite a different thing.

My system, which, for the past six months, has been in constant daily use on the English lines between London and Manchester, 200 miles, including nine miles of underground line, maintains synchronism within the 1,000th part of a second, for days at a time, without a second's interruption. On short lines, synchronism and octuplex telegraphy is entirely practicable—the variation never exceeding the 2,000th part of a second.

My system has been adopted by the English Government after exhaustive tests, and with full knowledge of Mr. La Cour's attempts and claims. Here, in Germany, I am now in negotiation with the Government for the use of my system. The La Cour system has been before them for more than seven years, and his instruments are among their shelved collection.

Mr. La Cour refers to the use of his system by Mr. Cassagnes, who is conducting experiments with steno-telegraphy between Paris and Marseilles. I know nothing of these experiments. I have received two inquiries by mail from Mr. Cassagnes, asking for terms under which my synchronism could be used by him, and I am credibly informed that in his prospectus Mr. Cassagnes bases his claims of advantage of his steno system upon the use of the Delany synchronism.

Mr. La Cour refers to pan-telegraphy incidentally, as if it was a trifling matter, easy of accomplishment by his system. But I have never heard o any results in this direction by Mr. La Cour, not even between Fredericia and Nyborg. The fact is, Mr. La Cour has not now, nor has he ever had any synchronism worthy of the name, and as for synchronous multiplex or fac-simile telegraphy, I don't think he knows anything about them. He has not, so far as I know, accomplished anything whatever in these directions.

It may be interesting to the INSTITUTE to know that I have recently obtained practically the most perfect synchronism by the system set forth in my patent 322,695, July 21, 1885, *i.e.*, bringing a yielding finger or spring attached to an electro-magnet armature in contact with the vibrator, so as to press against it. The pressure of the finger against the vibrator limits the amplitude of its vibration, and increases its rate. I have made a practical demonstration of it, not because my other methods were not good enough, but because the German Patent Office disputed my theory and required proofs. This system is entirely mechanical, so far as the reed is concerned.

Furthermore, I obtain perfect synchronism by an entire reversal of Mr. La Cour's theory, thus:

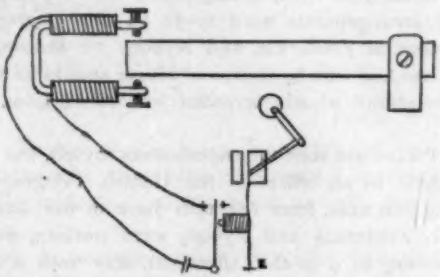

FIG. 5.

the charging of the correcting magnet, the poles of which are presented to the upper and lower edges of the vibrator *increases* the rate of vibration. The breaking of the circuit causes the lowering of the rate.

With my other independent correcting magnet the poles of which are projected towards the *sides* of the vibrator, thus:

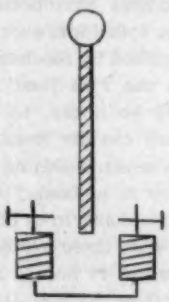

FIG. 6.

The greater the attraction of the correcting magnet the smaller the amplitude and the *lower* the rate of vibration. When the current is withdrawn the amplitude is increased and the *rate* also.

When the FRANKLIN INSTITUTE finds that I hold its high favors unworthily, I will be ready to surrender them, but the record of the INSTITUTE for fair and able investigation and discrimination leaves no apprehension in my mind on this head. Very respectfully,

PATRICK B. DELANY.

APPENDIX 3.

Direction des Télégraphes d'Etat de Danemark.

COPENHAGUE, le 9 Février, 1887.

Dear Sir:—In reply to your letter of January 24th, I have the honor to inform you that in the year 1880, during twenty-four days from the 17th June to the 10th July, arrangements were made on the telegraph offices of the Danish Government at Fredericia and Nyborg for sending two messages simultaneously over one wire by means of Morse apparatus and separate circuits produced by phonic wheels, invented by Poul La Cour, as synchronous instruments.

I regret that I have not seen the experiments myself, but I am in possession of a diary, held by an officer of the Danish Telegraph, whereby it is stated that during that time, from the 17th June to the 10th July, 1880, the phonic wheels in Fredericia and Nyborg were working every day from 8 or 9 in the morning to 9 in the afternoon, only with a few short interruptions caused by several events, such as disturbances on the telegraph wire or mechanical disturbances on the phonic wheels. For the period from the 22d June to the 10th July, it is expressly remarked that the synchronism was good.

I am further in possession of official reports, dated 28th June, 1880, from the head officers of the telegraph offices at Fredericia and Nyborg, after which the apparatuses on the both offices have been working regularly, and messages have been sent since the 17th June, except two days, when the working of the phonic wheels was disturbed by mechanical causes. In these reports it is especially stated that on the 27th June, 1880, were sent *over one wire*, from 9 hours, 57 minutes to 10 hours, 19 minutes simultaneously, four messages on each of the two circuits from Fredericia to Nyborg, eighty words on the one and seventy-seven words on the other circuit.

From 10 hours, 20 minutes to 10 hours, 36 minutes simultaneously, four messages on each of the two circuits from Nyborg to Fredericia, sixty-eight words on the one circuit, and sixty-three words on the other.

From 10 hours, 48 minutes to 11 hours, 2 minutes simultaneously, four messages (sixty-eight words) from Nyborg to Fredericia and four messages (sixty-five words) from Fredericia to Nyborg.

These messages, as well as the paper slips, such as received at the office of Fredericia, are in my hands.

After this, there is no doubt that in the year 1880 in this country, messages have been sent simultaneously over one wire by means of separate circuits produced by the phonic weeels, invented by Mr. Poul La Cour. Before that time there was not, nor has there ever since been any telegraph line in this country operated by synchronism as a multiplex system.

With great respect I remain,

[SIGNED] Very truly yours,

Director of the Danish Government Telegraphs.

MR. E. ALEX. SCOTT, Chairman of Committee of the FRANKLIN INSTITUTE, Philadelphia.

APPENDIX 4.

Ministere
des Postes et des Télégraphes.

Direction
du Materiel de la Construction.

2 Bureau. *A.*

Objet.

PARIS, le 26 Février, 1887.

Monsieur le Président:—Par lettre du 24 Janvier, vous voulez bien me demander si, avant 1881, on a réussi en France à augmenter la capacité de trafic des fils télégraphiques par l'usage d'appareils synchroniques à transmission multiple.

Je m'empresse de vous donner à cet égard les renseignements suivants:

(1°.) Dès 1858, Monsieur Rouvier, actuellement Directeur des Postes et des Télégraphes en retraite à Nîmes (Département du Gard), a imaginé un système de transmission multiple, comportant l'emploi de deux pendules rendus synchrones, et l'intercalation, entre les signaux d'une lettre Morse transmise par un poste, des signaux de lettres transmises par d'autres postes. Une description de ce système, faite par l'auteur a été insérée dans le recueil des *Annales Télégraphiques* de l'année 1860 (livraison de Janvier et Février, page 5): cette description a été rappelée et résumée dans un article sur la transmission multiple inséré dans les *Annales Télégraphiques* de l'année 1874 (livraison de Septembre–Octobre, page 187).

(2°.) En 1871, M. Meyer, agent de l'Administration francaise, aujourd'hui décéde, a réalisé un système de *transmission multiple*, fondé sur l'emploi de deux distributeurs circulaires parcourus par deux aiguilles synchrones, et sur l'intercalation, entre les lettres Morse transmises par un poste, des lettres transmises par d'autres postes. L'idée de la division du temps par des distributeurs circulaires synchrones avait été empruntée par M. Meyer à l'appareil typo-télégraphique de MM. Vairn et Fribourg, imaginé en 1864 et décrit dans le traité de télégraphie électrique de M. Rouvier de 1867 (Tome II, par. 839, pages 307 et 308).

Dans le système Meyer, l'impression des signaux Morse à l'arrivée s'effectuait à l'aide d'une hélice, disposée comme dans l'appareil autographique du mème inventeur, appareil qui avait figuré à l'Exposition Universelle de 1867.

L'article des *Annales Télégraphiques* de Septembre–Octobre, 1874, déjà cité renferme également la description de l'appareil multiple Meyer. La livraison de Juillet–Août, 1876, du mème recueil fait connaitre les perfectionnements apportés ultérieurement aux organes du système. Des appareils quadruples et sextuples qui functionnaient en service courant, sur un certain nombre de lignes françaises jusqu' à ces dernières années. Ils ont figuré à l'Exposition Universelle de 1876.

A l'Exposition d'Electricité de 1881, on remarquait encore un appareil de M. Meyer applicable au service simultané de plusieurs villes par un seul conducteur, un appareil multiple du mème à récepteurs indépendants, et un appareil multiple de M. Willot à récepteurs Morse.

(3°.) L'appareil multiple imprimant de M. Baudot, aujourd'hui en usage sur toutes les lignes françaises à grand trafic, brevet le 17 Juin, 1874, a figuré aux Expositions de 1878 et de 1881.

Agréez, Monsieur le Président, l'assurance de ma considération très-distinguée.

Le Ministere des Postes et des Télégraphes.

[TRANSLATION.]

Ministry
of
Postal Affairs and Telegraphs.

PARIS, 26th February, 1887.

Mr. Chairman:—In your letter of the 24th January, you ask me, if, prior to the year 1881, we have succeeded in France in increasing the working capacity of telegraph lines, by the use of synchronic apparatus for multiplex transmission.

I hasten to give you the following information on this point:

(1.) About the year 1858, M. Rouvier, now Director of Posts and Telegraphs, stationed at Nîmes (Department of Gard), conceived a system of multiplex transmission permitting the employment of two synchronized pendulums, and the interpolation between the signals of a Morse letter sent by one station, the signals of letters transmitted by other stations. A description of this system, made by the inventor, was published in the issue of the *Annales Télégraphiques* for 1860 (January and February, p. 85).

This description was revised and condensed in an article on "Multiplex Transmission," in the *Annales Télégraphiques* for 1874 (September and October, p. 187).

(2.) In 1871, M. Meyer, Government agent, since deceased, produced a system of multiplex transmission based on the employment of two circular distributors traversed by two synchronized hands (pointers?), and on the interpolation between the Morse letters transmitted by one station, letters transmitted by other stations.

The idea of the division of time by circular synchronous distributors, had

been borrowed by M. Meyer from the typo-telegraphic apparatus of Messrs. Vairn and Freibourg, invented in 1864, and described in the treatise on electro-telegraphy of M. Blavier, of 1867 (vol. ii, sec. 839, and pp. 307–308).

In the Meyer system, the impression of the Morse signals at the receiving station was accomplished with the aid of a helix, arranged as in the autographic apparatus of the same inventor—an apparatus which figured at the Universal Exposition of 1867.

The article in the *Annales Télégraphiques*, of September–October, 1874, already cited, also contains the description of the multiple apparatus of Meyer. The July–August number (1876) of the same periodical, described the final improvements applied to parts of the system. Quadruplex and sextuplex apparatus have been in regular service on a certain number of French lines up to the last few years. They figured at the Universal Exposition of 1878.

At the Electrical Exhibition of 1881, we noticed another apparatus of M. Meyer, applicable to the simultaneous service of several cities by a single conductor (live wire?), a multiple apparatus of the same, with independent receivers, and a multiple apparatus of M. Willot, with Morse receivers.

(3.) The multiplex printing apparatus of M. Baudot, now in use on all the important French lines, patented 17th June, 1874, figured at the Expositions of 1878 and 1881.

Accept, Mr. Chairman, the assurance of my most distinguished consideration.

GRANET,

[SIGNED] *The Minister of Postal Affairs and Telegraphs.*

APPENDIX 5.

Kaiserlich Deutsches Reichs-Postamt
II. Abtheilung.

BERLIN, W., 11 Februar, 1887.

Ew. Wohlgeboren theilt das Reichs-Postamt auf das gefällige Schreiben vom 24. Januar ergebenst mit, dass in der deutschen Reichs-Telegraphenverwaltung bereits vor dem Jahre 1881 und auch bis in die neueste Zeit hinein verschiedene Methoden des Gegensprechens und der Multiplex-Telegraphie, und zwar mit befriedigendem Erfolge, Verwendung gefunden haben.

(SIGNED) HAKE.

An HERRN E. ALEX. SCOTT, Vorsitzender des Committee of the FRANKLIN INSTITUTE, Philadelphia.

II. 1594.

[TRANSLATION.]

Imperial German Postal Department.
II. Division.

BERLIN, W. 11th February, 1887.

In reply to your letter of 24th January, the Imperial Post Office Department has the honor to state that various methods of simultaneous transmission (*Gegensprechen*) and of multiplex telegraphy have been employed in the

Imperial German Telegraph Service from a period prior to the year 1881, down to the present time, and with satisfactory results.

[SIGNED] HAKE.

To MR. E. ALEX. SCOTT, Chairman Committee of the FRANKLIN INSTITUTE, Philadelphia.

II. 1594.

APPENDIX 6.

To the FRANKLIN INSTITUTE for the Promotion of the Mechanic Arts, Philadelphia.

On the 7th of July, 1886, I had the honor of sending to the most honorable INSTITUTE a communication concerning my priority to the synchronism, that Mr. Delany had mentioned to the INSTITUTE as his own. As I have been informed of Mr. Delany's asserting to certain gentlemen in Europe, that my synchronism could not move at all, and that its moving had not been effected until done by means of his additions, I suppose that he is speaking in a similar manner to the FRANKLIN INSTITUTE. I therefore take the liberty of forwarding here enclosed a testimony of the perfectly working synchronism, for which I took out a patent in France on the 11th of October, 1882, the very same synchronism for which I took out a patent in England on the 9th of October, 1882.*

The paper is notified by the respective authorities at France.

I should value highly if the most honorable INSTITUTE would embody this paper, together with my former communication in the JOURNAL OF THE FRANKLIN INSTITUTE. I remain, yours very respectfully,

[SIGNED.] POUL LA COUR.

Askovhus, Vejen Station, Denmark, the 12th March, 1887.

A.

Ministere
des Postes et Télégraphes.

PARIS, le 4 Janvier, 1887.

Monsieur Cassagnes, Ingénieur civil, 18 Rue La Fayette à Paris.

Monsieur:—Conformément au désir exprimé dans votre lettre du 1 Décembre, je vous envoie ci—joint un extrait des rapports adressés à l'Administration, à la suite des expériences en ligne de votre système de Sténo-télégraphie.

Récévez, monsieur, l'assurance de ma considération distinguée.

GRANET.

Le Ministre des Postes et des Télégraphes.

Extrait—du rapport sur les expériences de transmission télégraphique faites au moyen du système Steno-télégraphique Cassagnes.

Dernière Série d'Expériences (*Octobre, 1886*).

La dernière série d'essais de steno-télégraphie multiple, faite en utilisant

* Letter and translation appended, and marked A.

la composition préalable et la transmission automatique, a donné les résultats suivants.

On peut transmettre par minute et par secteur de 125 à 145 mots; quant au nombre de transmissions, ou secteurs, il dépend de la résistance et de la capacité dè la ligne. Un appareil triple, ou à 3 secteurs, pourrait être utilisé jusqu' á 500 ou 600 kilomètres, un appareil double jusqu' á 700 ou 800, ou peut être au delà; mais dans les essais qui ont été faits sur un circuit de 900 kilomètres, la correction n'était pas toujours efficace et il en résultait des interférences nuisibles. Le rendement par heure serait donc de 25,000 mots environ entre Paris et Lyon, et de 15,000 à 16,000 sur de plus longues lignes, toutes réserves faites en ce qui concerne le langage sténographique qui forme la base du système.

Je certifie pour copie conforme l'extrait du rapport de l'Aministration française des Télégraphes qui précède.

Je certifie de plus que dans mes appareils sténo-télégraphiques dont les résultats *en ligne* sont officiellement constatés ci-dessus, le seul synchronisme dont j'ai fait usage—et qui n' a cessé de fonctionner de la manière la plus satisfaisante sur des circuits français de (800) huit cents kilomètres—est le synchronisme par la roue phonique de Monsieur Poul La Cour d'Askovhus (Danemark) tel qu'il été breveté en France par Monsieur La Cour le 11 Octobre, 1882.

[SIGNED] A. CASSAGNES.

Paris, le 2 Mars, 1887.

A

[TRANSLATION.]

Ministry of Postal Affairs and Telegraphs.

PARIS, the 4th January, 1887.

Monsieur Cassagnes, Civil Engineer, 18 Rue LaFayette, Paris.

Sir:—Conformably to the desire expressed in your letter of the 1st December, I send you annexed an extract from the reports addressed to the Government, in consequence of the experiments on the line of your system of steno-telegraphy.

Receive, sir, the assurance of my distinguished consideration.

GRANET.
The Minister of Postal Affairs and Telegraphs.

Extract from the reports on the experiments in telegraphic transmission, made by means of M. Cassagnes's steno-telegraphic system.

Last Series of Experiments, October, 1886.

The last series of experiments in multiplex steno-telegraphy, made by utilizing the previous composition, and automatic transmission, has furnished the following results:

We can transmit per minute and per section from 125 to 145 words; as for the number of transmissions or sections, it depends on the resistance and the capacity of the line. A triplex apparatus or of three sections, might be utilized up to 500 to 600 kilometres (310,675 miles; 372,810 miles), a duplex apparatus up to 700 or 800 kilometres (434.945 miles; 497,080 miles), or perhaps beyond; but in the experiments made on a circuit of 900 kilometres (559.215 miles), the correction was not always efficacious, and injurious interferences resulted. The accomplishment per hour would then be about 25,000 words between Paris and Lyons, and from 15,000 to 16,000 on longer lines, every reservation being made as to what concerns the stenographic language forming the basis of the system.

I attest, as a certified copy, the preceding extract from the report of the French telegraph administration.

I certify, moreover, that in my steno-telegraphic apparatus, the results of which on the line are officially stated above, the only synchronism of which I have made use, and which has not failed to operate in the most satisfactory manner on French circuits of 800 kilometres, is the synchronism by means of the phonic wheel of Monsieur Poul La Cour of Askovhus (Denmark) as patented in France by M. La Cour, the 11th October, 1882.

A. CASSAGNES.

Paris, 2d March, 1887.

March 28, 1887.

To Mr. POUL LA COUR, Askovhus, Vejen Station, Denmark.

Dear Sir:—Acknowledging the receipt of your communication of March 12, '87, with enclosure from the Ministere des Postes et Télégraphes, of Paris, I have the honor to inform you that this communication, as well as your earlier one, of July 7, '86, has been referred to a special committee for proper investigation. The committee has given the subject careful examination, and the report thereon is expected to be ready for presentation at the next stated meeting of the INSTITUTE (April 20, '87).

The whole subject will undoubtedly appear in the JOURNAL as soon as the Committee's Report shall have been formally acted on by the INSTITUTE.

I have the honor to remain your obedient servant,

[SIGNED] WM. H. WAHL, *Secretary*.

STUDIES ON THE STRATIFICATION OF THE ANTHRACITE MEASURES OF PENNSYLVANIA.

By HENRY A. WASMUTH, Instructor of Mining, University of Pennsylvania.

The State Geological Survey advocates the theory, that the anthracite measures have been folded into numerous "inversions," without fracture of the strata.

The general theories of "faults," established by long experience in mining in Europe, are described and advocated in the *Report of the First Geological Survey of Pennsylvania.*

On the strength of the general theories of faults above mentioned, and my long experience in studying and developing faults, varying much in character and extension, I maintain:

That in "bedded" mineral deposits no "inversion" or "overlapping" of the strata can take place, without fracture and more or less dislocation: and that, in general, the dislocations of the strata take place in one of two ways; either the portion of a mineral deposit on the hanging wall of the fracture or fault is in a lower position, than the portion on the foot wall, as illustrated in Fig. 1, or it is in a higher position, as illustrated in Fig. 2. Occurrences, as illustrated in Fig. 1, are called "transverse faults;" occurrences, as illustrated in Fig. 2, are called "longitudinal faults," or overlaps.

In order to explain this matter conveniently, I will first quote some extracts from the *Reports of the First and Second Geological Survey of Pennsylvania*, and then illustrate and describe a number of faults developed in anthracite mining, and apply the facts, established thereby, in the examination of some of the sections of the *Second Geological Survey of Pennsylvania.*

EXCERPTS.

The researches of the Geological Survey, and the experience, often very dearly purchased, of the conductors of our anthracite mines, have at least induced a very general conviction of the necessity of attending to all the anticlinal and synclinal turns of the strata, for it is now admitted that these are the true key to the opening, tracing and successful working of the coal seams of the region, etc.*

* *First Geol. Survey of Penn, ii,* 28.

Contortions of the coal beds unaccompanied by fracture, are very numerous in some portions of the coal field. Where the flexure reaches its maximum degree, the short leg of the anticlinal becomes of course inverted, and then the coal, in all this part of the bed and at both the anticlinal and the synclinal elbows, is in a compressed and fragmentary state. There are other irregularities of the coal arising from a sudden change of inclination, once or oftener, within a small space. Such usually announce proximity to a crushed axis, or, what is the same thing, to a longitudinal dislocation, which is but the extreme limit of the displacement, that, towards its termination closes, and becomes a mere fold or axis.*

The actual displacements or breaches of contact in the coal may be divided into two classes; one, embraces every form of disturbance in the coal bed parallel to its dip; the other, every variety of cross fracture. The first might, for convenience, be called *parallel slips*, the others are named by their usual title of *faults* or *cross fracture*. In very many instances, the parallel slips present an extensive disturbance of the mass of the coal bed within itself, without more displacement of the roof and floor than amounts to a series of local bulgings and contractions of the layer. *The coal has rubbed upon itself, and is in a more or less fragmentary, friable and polished state.* To admit of this parallel displacement or moving forward of one part of the bed upon the other, there must have arisen somewhere, it is obvious, *either a fracture or a doubling upon itself of the roof, or the floor, or of both.* It has been already shown that the sharp mountain beds illustrate to an extraordinary degree this form of displacement by parallel slipping.

Not infrequently the miner comes upon fractures in the roof or the floor of his coal bed, which explain to him the origin of local parallel dips. In this cases, the one boundary of the bed giving way, and not the other, the forward movement of the coal causes one of the broken portions of the roof or floor *to pass by or overlap* the other, and even to bend and curl up under the prodigious pressure applied to it. This is exemplified in the accompanying section of a disturbance of this nature in the great bed of Lorberry Creek, where the floor has broken, become involved in the sliding coal, and curved back upon itself until it has met the roof. At

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* *First Geol. Survey of Penn., ii*, 236.

this point the bed, the proper thickness of which is about twenty-seven feet, has been swollen by the overlapping of its splintered ends to a size of fifty feet. *Fig. 2A.*

Of the other class of displacements, *the cross fractures or true faults*, little need be said here in detail. *Where they range with the strike* or course of the strata, they will be found *to terminate generally in anticlinal folds or flexures, first closely compressed and crushed, but farther on becoming more regular*, and at last dying out into the general dip. *Such longitudinal faults bring a rock wall against the lower end of the coal bed*, and in each instance where a *valuable vein* is thus abruptly cut off, it becomes a problem of much practical interest to determine the direction in which the last portion of the coal bed is to be sought.*

It may be accepted as a general rule, not without its occasional exceptions, that these longitudinal faults consist of *an upthrow of the vein to the south of the fault*, so that the remainder of the vein, if we are mining downward, will be recovered at a point above and further back towards the outcrop, compared with the spot where the rock wall crosses the mine.

Tracing such a fault through its successive stages, we shall generally be able to resolve it into a broken, oblique flexure, and then in a gentle, normal one. The three conditions of the dislocated flexure are illustrated in the diagrams here annexed, *Fig. 2B.* Another species of cross fracture or dislocation is where *the break is transverse to the strike or course of the strata*, sometimes perpendicular to it, sometimes oblique. These displacements bring a rock wall abruptly against the vein in the direction of its length; or, in other words, at the end of a gangway or level, and only *where they are oblique do we encounter them also in the direction of the dip.*

When traced for any considerable distance across the strata, such a fault will be found to be connected with a difference in the slope of the bed upon its two sides, the one portion having been more uplifted than the other, and therefore shoved forward beyond it. These are all instances to be explained by some inequality in the transverse pressure, propagated northward or southward to the dislocated strata; and this inequality will usually be found to

* *First Geol. Survey of Penn., ii*, 237.

proceed either from the sudden rising of an anticlinal flexure to the north or south of the position of the fracture, or from a sudden termination of a longitudinal dislocation.

Where an obliquely transverse fault divides the strata, we shall usually discover that the most uplifted set of rocks has been shifted forward beyond the termination of the other less inclined portion.

By this means, the shapes of the rolls and overturns are exhibited, and means are afforded to mining engineers and superintendents for directing their headings with less anxiety respecting what is in advance of them.

The method of constructing cross sections, by Mr. Ashburner, will be closely criticised by the mining engineers of the region. I am convinced that they will not only respect the conscientious carefulness, but more and more admire the scientific skill with which this part of the work has been done. I think that they cannot help feeling a considerable confidence in the probality of the construction wherever there is a lack of actual data. At all events, many new and useful ideas will be contributed to the engineering science of the region.

During the latest mining excursion, the following-named collieries have been visited:

Kohinoor	Colliery,	controlled by the	P. and R. C. and I. Company.
Middle Creek	"	"	"
Otto	"	"	"
East Franklin	"	"	"
West Brookside	"	"	"
Williamstown	"	"	Penna. R. R. Company.

Kohinoor Colliery.—The Mammoth and Holmes beds have been opened by two separate shafts within a short distance. The Holmes bed dipping about 12° south, has been cut off by a longitudinal fault in the west gangway. For the recovery of the lost portion of the bed, a cross cut towards the hanging wall was commenced as illustrated in *Fig. 3*. The rock through which the cross cut passed is parallel to the Holmes bed for a distance of about twelve to fifteen feet and then a distinct fracture or fault, dipping about 45° south, has been intersected. The width of the fault is about two inches, and it is filled with disintegrated rock and coal.

Fig. 1.

Fig. 2.

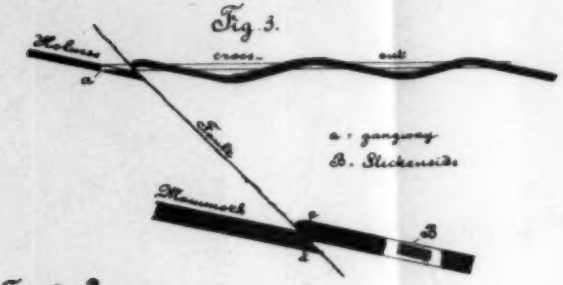


Fig. 3.
a = gangway
B. Slickensides

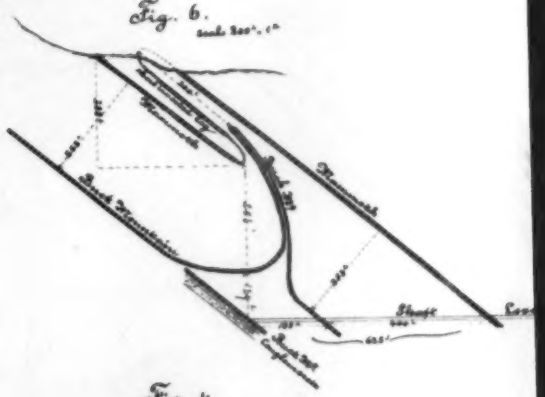


Fig. 6.

Fig. 2. A.

Fig. 2. B.

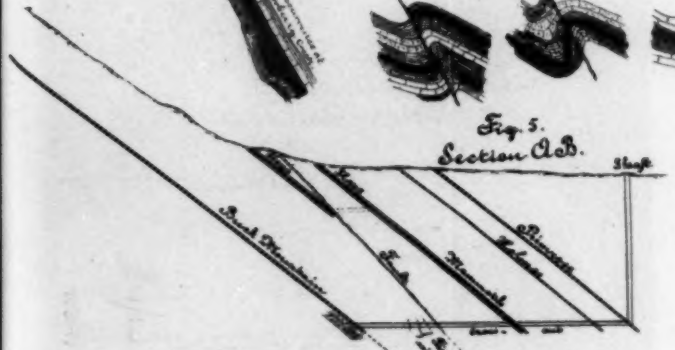


Fig. 5.
Section A B.

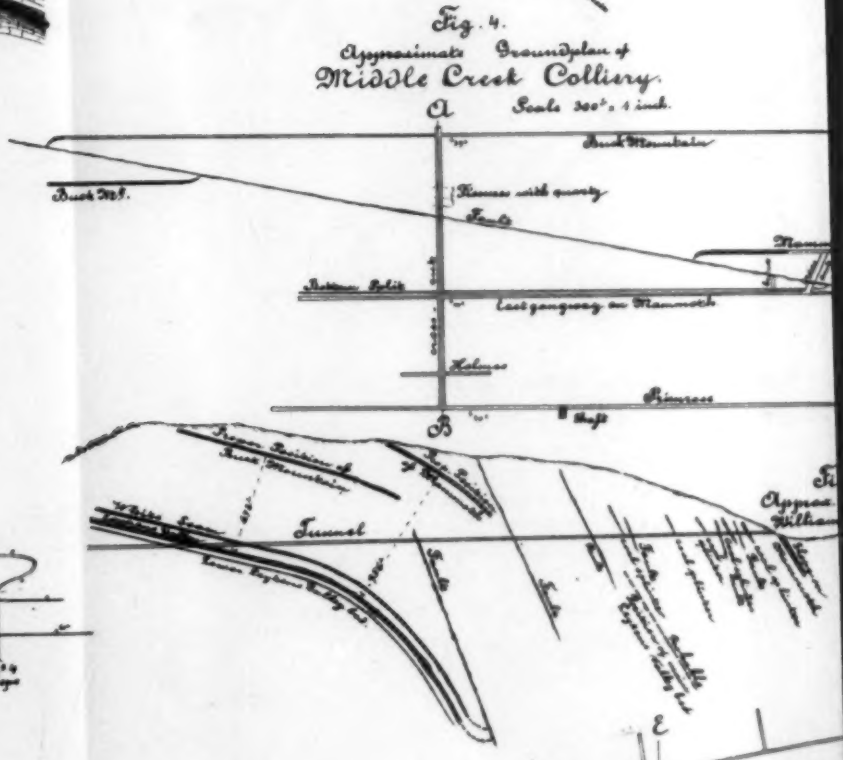


Fig. 4.
Approximate Groundplan of
Middle Creek Colliery.
Scale 200 ft. = 1 inch.

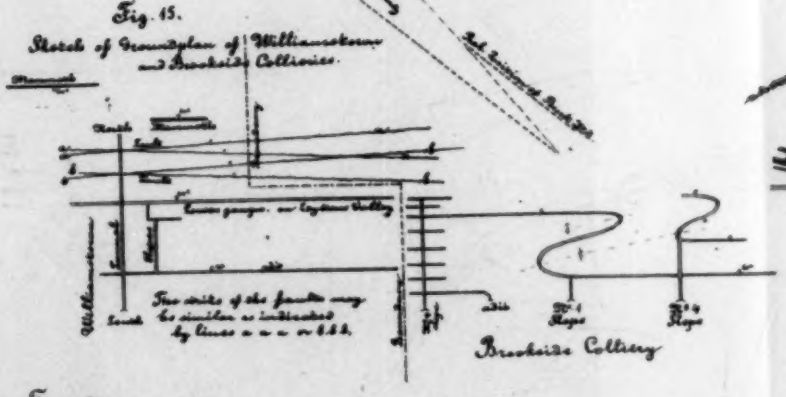


Fig. 15.
Sketch of Groundplan of Williamstown and Brookside Collieries.
Brookside Colliery

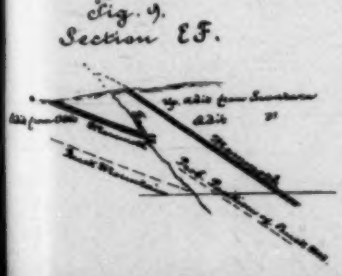


Fig. 9.
Section E F.

Fig. 8.
Section C D

Fig. 6.

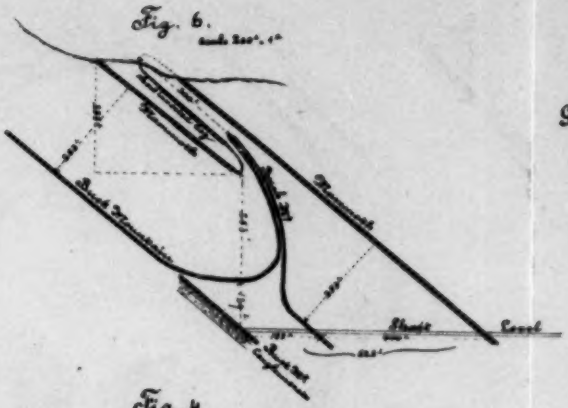


Fig. 10.

Approx. Groundplan of East Fra

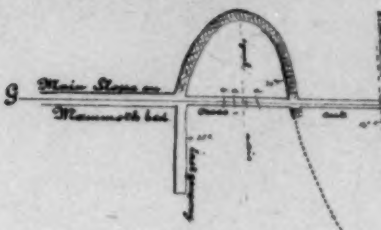


Fig. 11.

Section GH.

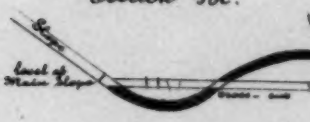


Fig. 4.

Approximate Groundplan of

Middle Creek Colliery.

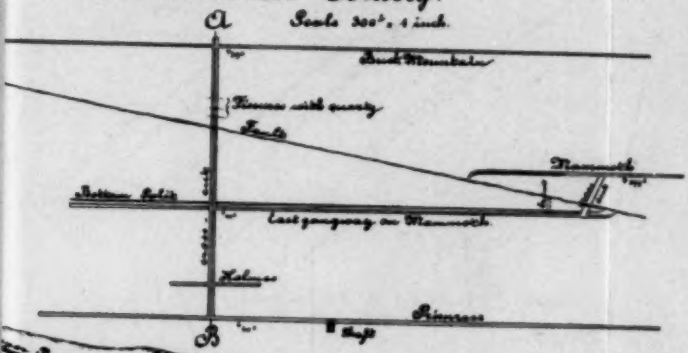
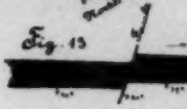


Fig. 12.

Fig. 13



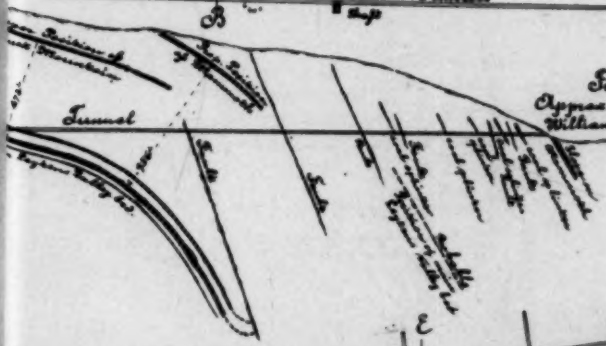


Fig. 16.

Approx. Section of Williamstown Tunnel.

Scale 200 ft. 1 in.

Fig. 7.

Approximate Groundplan of

Otto Colliery.

Scale 6 ins. 1 m.

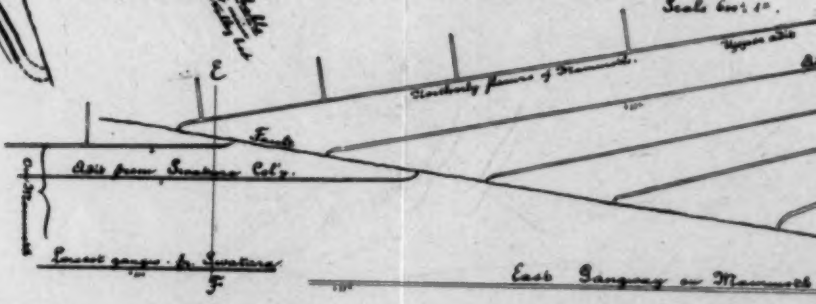


Fig. 10.

of East Franklin Colliery.

Fig. 14.

Fig. 32.

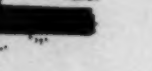
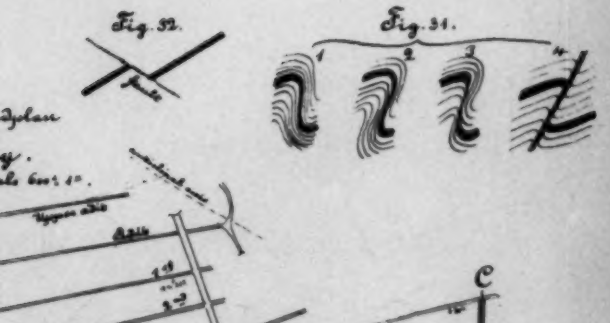
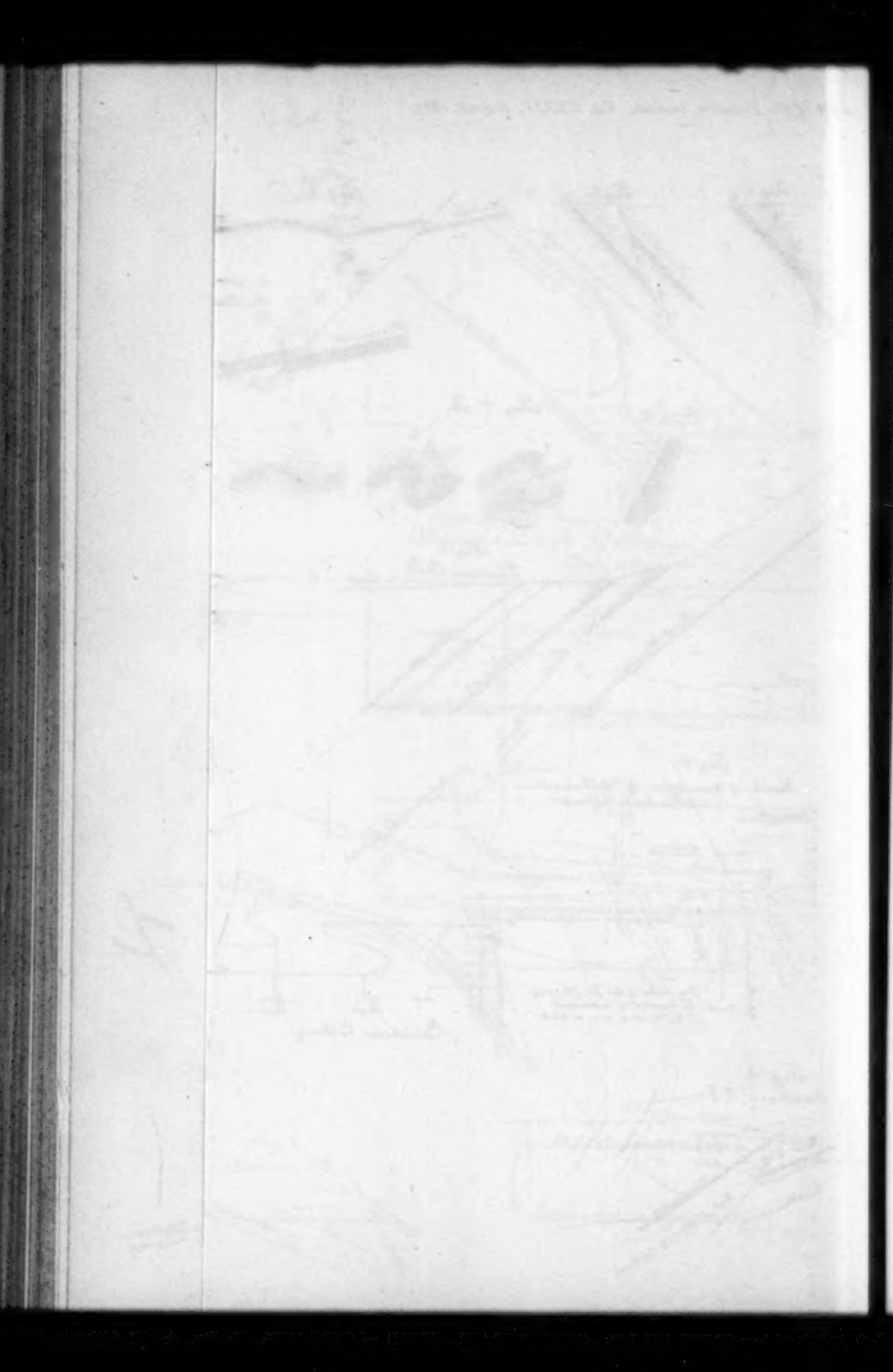


Fig. 31.





SECTION Nº 2. FROM WESQUEHONING VALLEY TO MAUCH CHUNK VALLEY THROUGH KNUNE RUN (Nº 1) TUNNEL.
Bearing S. 8 E.

Fig. 17.

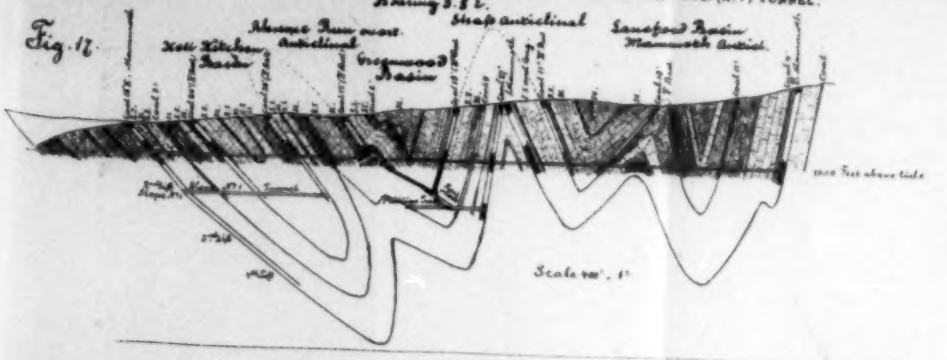


Fig 18. SECTION Nº 3. THROUGH TUNNEL Nº 2.

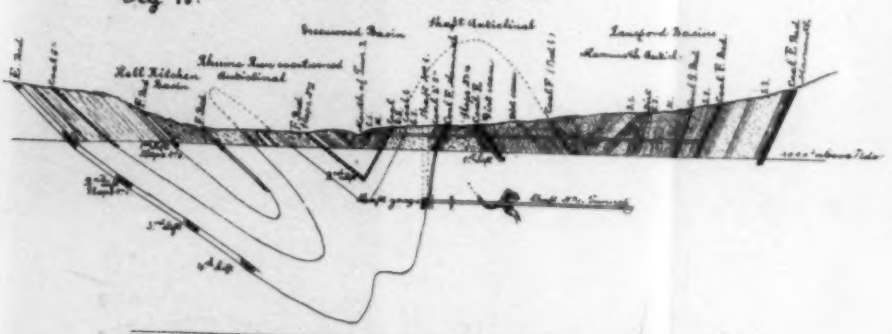


Fig 19. Section along Meridian Line 20000' West of Mauch Chunk about 1800' East of Nesquehoning Tunnel. (Vol. I. 3rd Geol. Surv.)

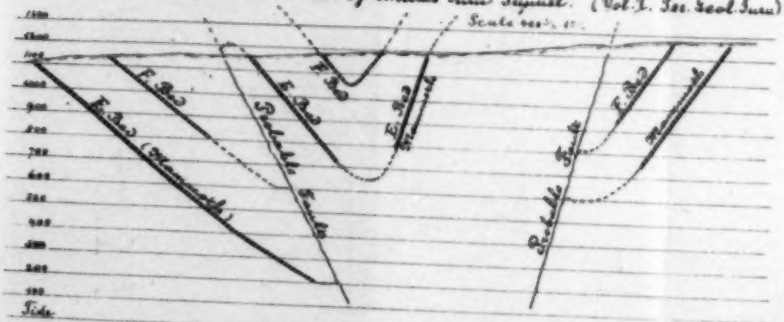


Fig. 20. Section along Meridian Line 22000' West of Mauch Chunk about 400' West of Nesquehoning Tunnel.

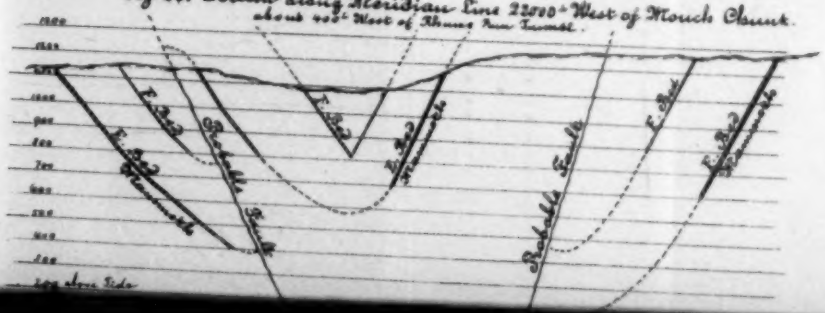


Fig. 21. Se

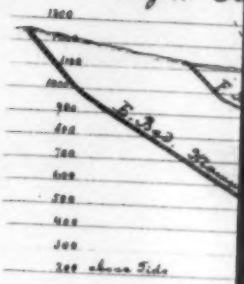


Fig. 22. Se

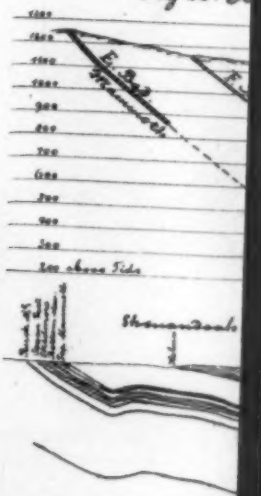


Fig. 26.

Fig. 21. Section along Meridian Line 24000ˢ West of about 900ˢ West of Tunnel N° 2.

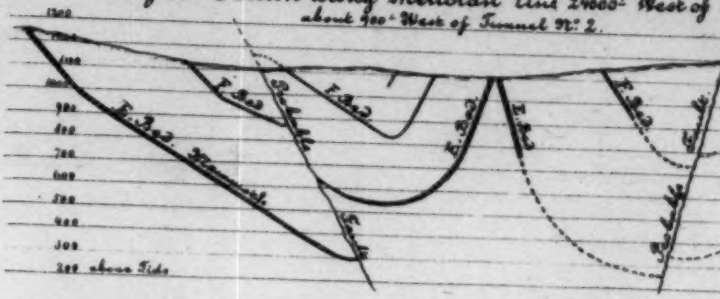


Fig. 22. Section along Meridian Line 26000ˢ West of Mo

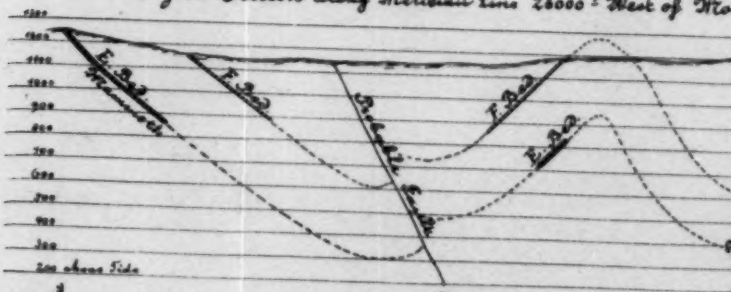
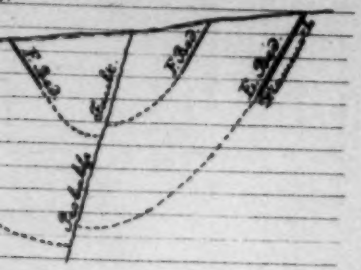


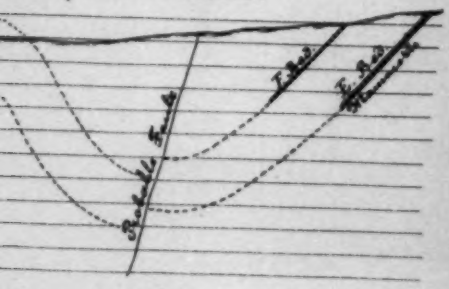
Fig. 25. Geological Section along Line

Fig. 26. Geological Section along Line C D.

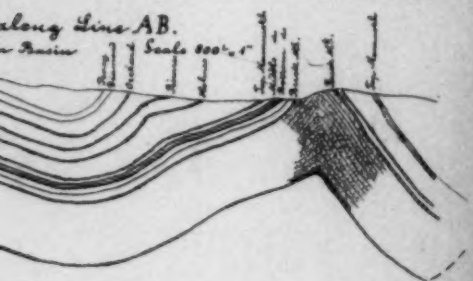
000' West of Mauch Chunk.



West of Mauch Chunk.



along Line AB.



Line CD.

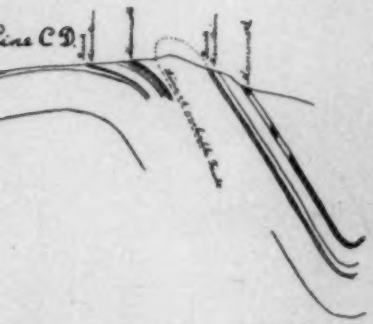
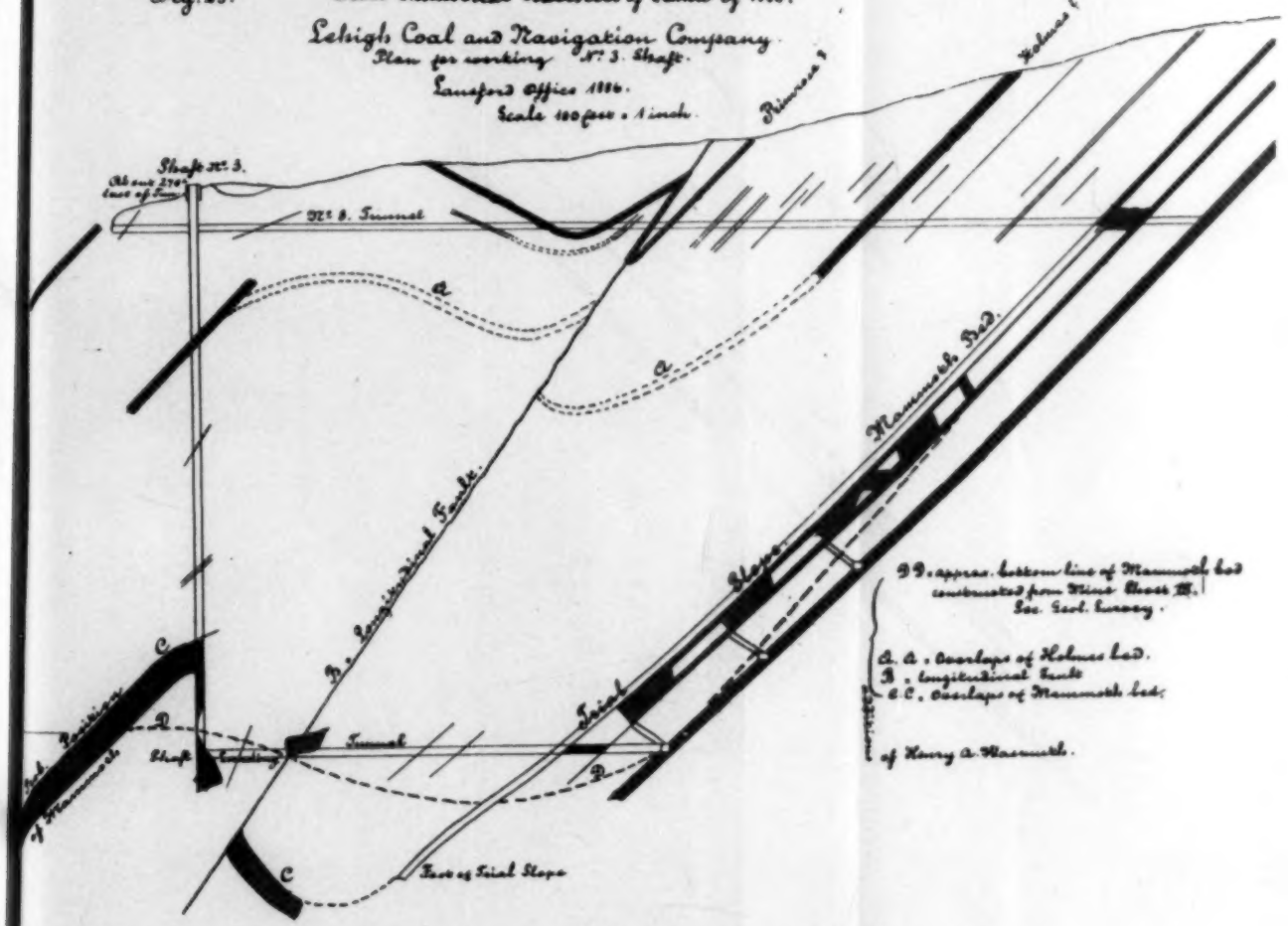


Fig. 23. From Industrial Statistics of Penna of 1885.

Lehigh Coal and Navigation Company.
Plan for working No. 3. Shaft.
Lansford Office 1886.
Scale 100 feet = 1 inch.



99. appros. bottom line of Mammoth bed constructed from Mine Sheet 28, Sec. Geol. Survey.

A. A. Overlaps of Holmes bed.
B. Longitudinal Faults
C C. Overlaps of Mammoth bed.

Section of Henry & Mammoth.

Fig. 24. Lehig

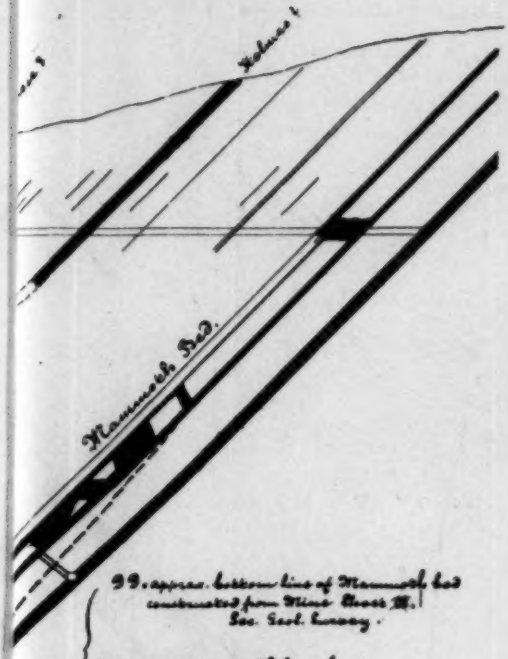
Level of Tunnel No. 6

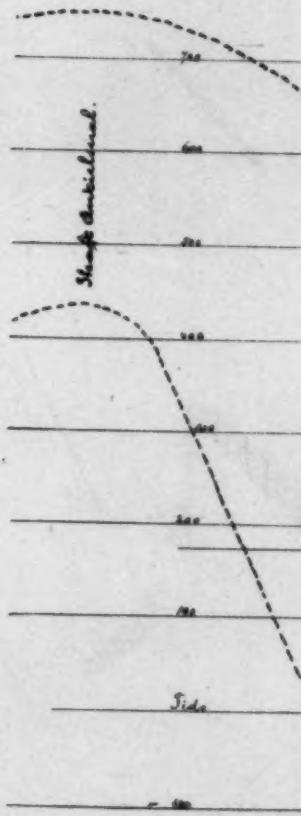
Mammoth Anticlinal.

Tide

Fig. 24. Lehi

Level of Tunnel No 6.





99. appros. bottom line of Mammoth bed
contoured from Mine Sheet III.
See Geol. Survey.

A. A. Overlaps of Holmes bed.
B. Longitudinal fault
C. C. Overlaps of Mammoth bed.

of Henry A. Mammoth.

From Industrial Statistics of Penna of 1885.

Lehigh Coal and Navigation Company.

Shaft No 2; colliery No 6.

Surveyed Office Feb. 1885.

Scale 100 ft. = 1 in.

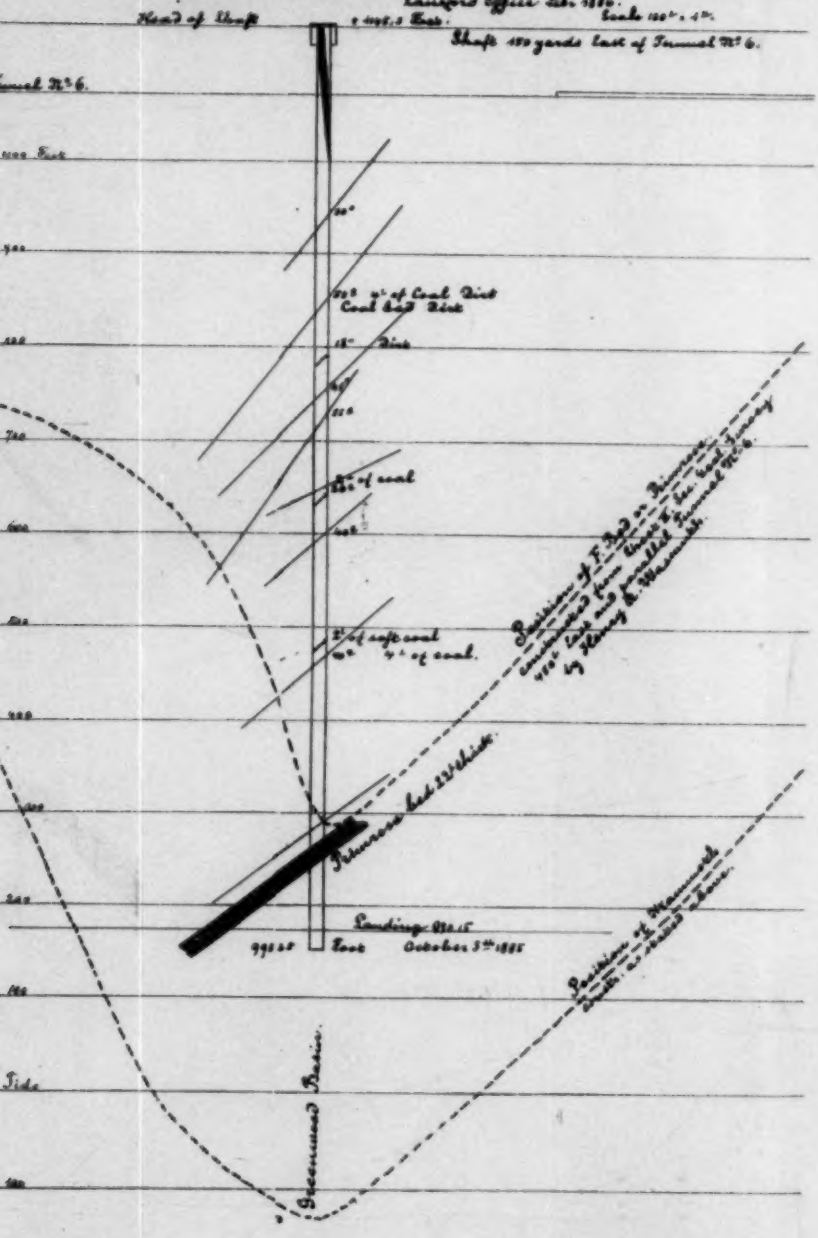


Fig. 27. Western Middle Coal Field. Mine Sheet No 11. Sec. Geol. Survey
Section along prolonged Line C D.
Scale 400' = 1".

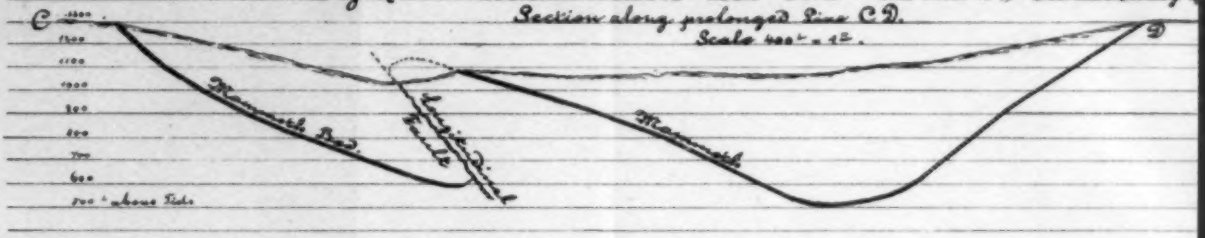


Fig. 28. Section A B through A and parallel C E.

Fig. 29. Section F G through Kobinoor Shaft para

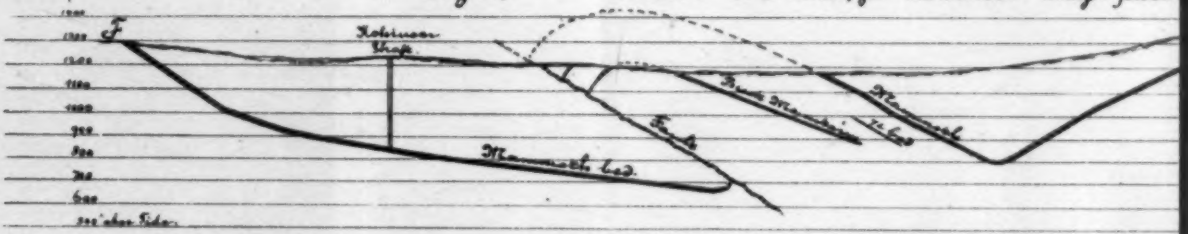
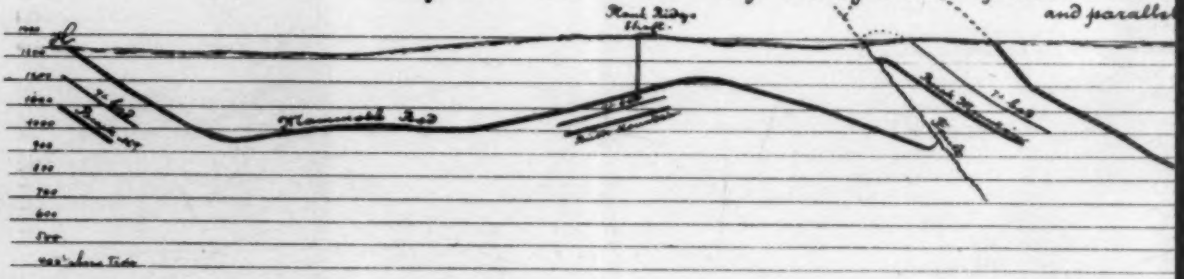
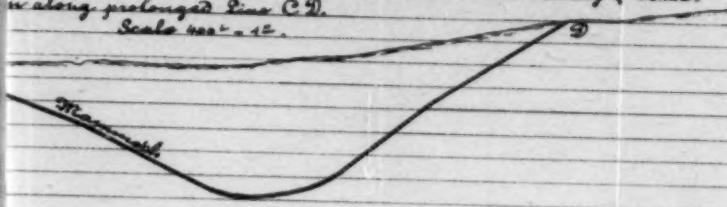


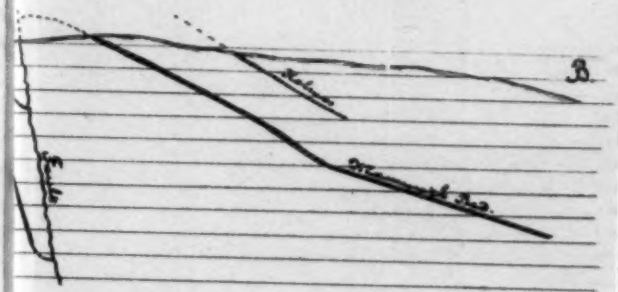
Fig. 30. Section H I through Kehley Run Slope Mouth and
and paralle



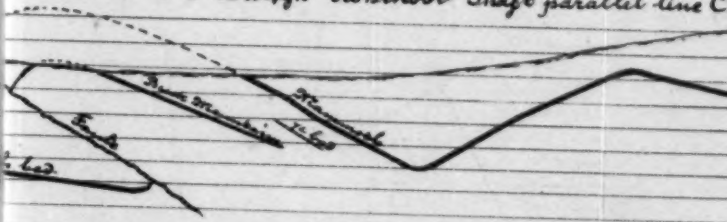
Middle Coal Field. Mine Sheet No II. See Geol. Survey of Penna.
w along prolonged Line C D.
Scale 400ft = 1in.



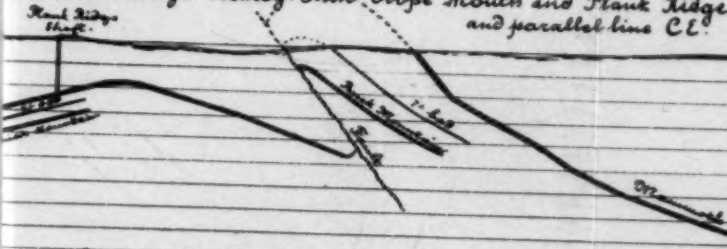
ion A B through A and parallel C E.

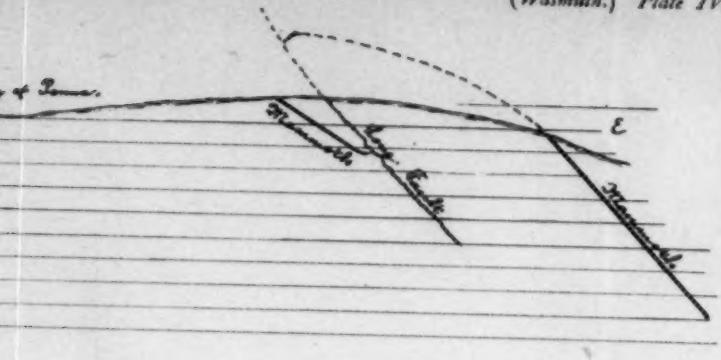


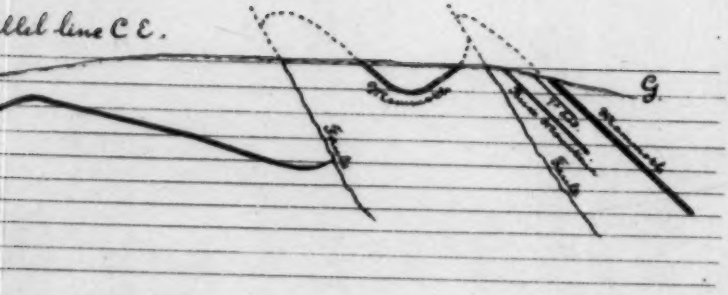
Section F G through Kohinoor Shaft parallel line C

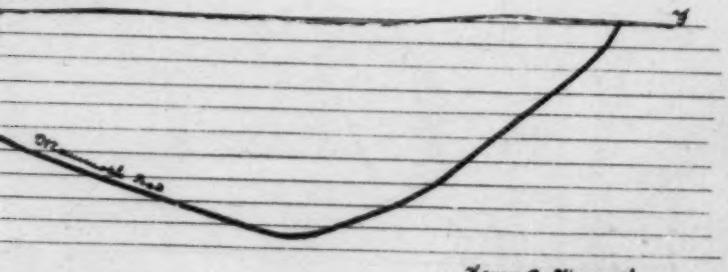


ion H I through Kebley Run Slope Mouth and Plank Ridge
and parallel line C L.









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Instructor of Mining
University of Penna.

The bottom strata of the lost portion of the Holmes bed and parallel to its direction, have been struck directly on the hanging wall of the fault, and at the same time a gentle north-dipping flexure of the Holmes bed (anticlinal elbow), which terminates on the fault in the higher portion of the cross cut. The north-dipping flexure referred to, assumes a southerly dip of about 10° to 12° within a short distance, and sets down to the level of the cross cut or gangway, showing here its proper thickness. The bed forms several synclinals and anticlinals in its continuation, which have been intersected by the cross cut. [Statement of the mine boss.] The overlapping on the Holmes bed on the incline is about twenty to thirty feet. There is no room for even a suspicion of the existence of a short, inverted leg of the coal bed, *Fig. 3*, because if the fracture should represent the short leg, certainly there would be signs of parallelism of the coal bed and of its country rocks, too.

The dislocation of the Holmes bed necessarily must have been accompanied by dislocation of the lower Mammoth bed by the same fault, thus increasing the thickness of about twenty-three feet to about fifty feet as represented by line *c*, *d*, *Fig. 3*. The gentle rollings of the Holmes bed referred to, already have been or will be developed similarly in the workings of the Mammoth bed.

Numerous fractures of different dip and strike have been met with in the workings of the Mammoth bed, and also several gentle rolls. "A slickensides," about 2½ by 2 feet, has been exposed on the left rib of a gangway, on a gentle rising anticlinal, *B*, *Fig. 3*, the striation of which indicates a movement of the strata, obliquely to the lamination of the coal bed, thus strengthening the conjecture, that the local thickness of the Mammoth bed of about fifty feet near this striation has been produced by pushing and tossing of the gentle rolls of the strata *into numerous overlaps* of small extent by prodigious forces.

Middle Creek Colliery.—This colliery is one of the oldest operated in the Tremont district, and has been announced as having developed an "inversion," by its sinking, of two slopes on two different south-dipping flexures of the Mammoth bed within an horizontal distance of about 156 feet from knuckle to knuckle, and in an almost vertical plane. The ground plan of the mine, *Fig. 4*, has been constructed from data of memory, and from this section *Fig. 5*.

In the northern slope (*Fig. 5*), the coal bed terminated on rock at the depth of about 480 feet. The southerly slope, it has been proved, is on the same flexure of the Mammoth bed, worked from the level of the shaft.

The phenomenon has been explained to be an "inversion," as illustrated in *Fig. 6*, by all experts and officers in charge ever since. Assuming that the inversion exists here, then, according to the notions of the engineers, etc., as well as the illustrations of the State Geological Survey, the lower Buck Mountain bed must be inverted also, as illustrated in *Fig. 6*. The strata between the Mammoth and Buck Mountain beds is about 283 feet in Eckert Colliery near Tremont.*

In order to accommodate the doubling of the Mammoth and Buck Mountain beds, the strata of twice 283 feet = 566 feet at its greatest thickness, and considerable portions of the conglomerates of the bottom strata of the Buck Mountain bed also must have been almost squeezed out of existence.

If the rocks at the time of their disturbance have been as hard as they are to-day, is it supposable that such an overlapping could take place?

The persistency with which the present officers in charge insist that there is an inversion of the Mammoth bed, compared with the condition of the strata developed in mining, only permits the explanation that all the strata down to the Buck Mountain bed must have been pulled up and then shifted into a long, very narrow inversion.

But it has been proven beyond any doubt that no inversion has been developed in this colliery thus far, for: from the lowest or bottom gangway of the northern slope (*Fig. 5*) a cross cut toward the hanging wall has been driven for ventilating purposes, etc. It has not been reported yet that this cross cut has intersected the short, inverted legs of either the Mammoth or the Buck Mountain beds and their country rocks. An investigation of this cross cut was excluded, on account of water at the time of our visit.

From the bottom level of the shaft, which is about 700 feet below the levels of the knuckles of the slopes referred to, a cross cut has been driven from the Primrose to the Buck Mountain bed,

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* *Second Geological Survey of Pennsylvania.*

which crossed the Mammoth bed. About 308 feet north of the Mammoth bed, a distinct fault, filled by disintegrated coal and rock has been intersected by said cross cut, and within about seventy-seven feet farther north, several irregular fissures filled or lined with quartz. In this portion of the cross cut, the rock is very hard and without distinct stratification, but from here until the Buck Mountain bed has been intersected, the dip of the strata is about from 37° to 39° south. According to the theory on overlaps, advocated by the engineers of the anthracite region, as well as the illustrations of the State Geological Survey, the Buck Mountain bed should have been crossed at about 440 feet $\left(\frac{283}{\sin 40°}\right)$ *Fig. 6*, but the cross cut is about 625 feet long; that is, from the bottom of the upper member of the Mammoth to the bottom of the Buck Mountain bed. The greater length of the cross cut, about 185 feet, may be accounted for by a horizontal dislocation of about 156 feet, and the remainder by the local decreasing dip of the strata.

The non-existence of an inversion has been developed also by the east gangway on the Mammoth bed, illustrated in *Fig. 4*. The coal bed became contracted and, turning into a northerly course, it terminated completely. After several trial cuts, the top rock of the Mammoth bed (hard sandstone) and the lost portion of the bed was recovered by an oblique cross cut, about 160 feet long. The distance is about equal to the distance between the mouths of the two slopes on the Mammoth bed referred to, and also nearly equal to the distance of dislocation in the main cross cut. In the outside breast of the oblique cross cut, the coal terminated just as in the gangway, but I do not recollect at what height on the dip. A close investigation of the occurrence was prevented by the supporting timbering of the mouth of the oblique cross cut.

The east gangway on the Primrose has been stopped for some time, most probably in the same fault.

It has been demonstrated frequently, that such longitudinal faults seldom carry much water, but a great deal of mine gas on the strata of the hanging wall of the fault, which is explained by the shape of the flexure of the strata. This has been experienced to an extraordinary degree in the workings on the Primrose and Mammoth beds above shaft level.

The Otto Colliery.—Has operated during a long time above and below water level, two south-dipping flexures of the Primrose and Mammoth beds within a horizontal distance of about 1,300 feet.

The northerly flexure of the Mammoth bed, then called "White Ash Vein," has been worked by two adits above each other in a westerly direction for about one mile, and here cut off by rock. The very termination of the bed also has been reached at three lifts below water level. In the third level below water, the bed turned south and terminated about 350 yards west of the slope, *Fig. 7.* The ground plan of the mine has been constructed from memory. *Figs. 8* and *9* represent sections of it.

The southerly flexure of the Primrose and Mammoth beds have been operated in a westerly direction from Otto Colliery, and in an easterly direction from Swatara Colliery above and below water level to about equal horizons. The adits from Swatara Colliery on the Mammoth bed have been extended eastward for about 700 yards, then the bed turned towards the north and terminated on rock. The distance between the terminations of the corresponding water levels on the southerly flexure from Swatara Colliery, and on the northerly flexure from Otto Colliery is reported to have been very small, and their relative positions are explained to be on the contorted flexures of several anticlinal rolls, and as many synclinal basins.*

The east gangway on the southerly flexure of the Mammoth bed in the then lowest level of Otto Colliery had struck a fault with southeasterly strike. In this fault a so-called monkey gangway has been driven for more than 100 yards without recovering the lost portion of the Mammoth bed, *Fig. 7.* Then the fault was supposed to be an inversion, and they expected to recover the lost portion of the bed towards the north within a short distance; accordingly, a cross cut was commenced about 125 yards west of the intersection of the fault, *Fig. 7.* The narrow dimensions of this cross cut at the beginning confirm the stated supposition, yet to be investigated. The cross cut intersected regular south-dipping strata for about sixty feet from the gangway and then broken rocks, being about twelve feet thick in the upper part of the cross cut, and about four feet at its bottom. A distinct stripe of disintegrated rock, like a bench of yellowish clay, similar to a fault, is

* *First Geological Survey of Pennsylvania, i,* 434.

visible about the middle of the broken strata. The lower (synclinal) elbow of a contorted flexure of a thin coal bed is intersected just north of the broken strata, which terminates like the latter. From here, regular south-dipping strata only, with a number of coal beds, has been intersected by the cross cut and at about 1,350 feet from foot wall to foot wall, the then so-called "New Vein," or the lost portion of the Mammoth bed, identical with the northern flexure of the Mammoth bed, above referred to. The phenomenon has been defined to be an inversion by mining experts, and the officers in charge ever since (the fault was struck about twelve years ago), but, as we were informed by a very eminent mining expert at the time of our visit, it has not yet been clearly determined.

That the phenomenon is a longitudinal fault of considerable extent, is shown so distinctly as not to allow even a conception of an existing inversion; for, if it were an inversion, then the short, inverted legs of strata, not less than 600 to 700 feet thick, and including a number of naturally deposited coal beds, must have been compressed to a thickness of about four feet, or to the width of the broken strata referred to.

East Franklin Colliery.—From the bottom of the main slope, a narrow synclinal and an anticlinal have been developed within a distance of about 345 feet across the strata, *Fig. 10.* A number of oblong quartz druses, more exactly clefts, proofs of disturbance of the strata, have been intersected by the cross cut, *Figs. 10* and *11.* One of these clefts forms an outlet for considerable quantities of mine gas, thus proving a connection with over- or under-lying coal beds. The northerly flexure of the anticlinal is intersected by a distinct fracture, dipping about 70° north, and filled with disintegrated coal and rock, similar to a longitudinal fault. The coal of both flexures of the anticlinal above the level of the cross cut, is in a very fragmentary, friable state, running like quicksand, and perhaps, on account of this, the extension of dislocation by the fault referred to, has not yet been looked for. The sudden and great increase of the dip of the northern flexure of the coal bed, compared with the gentle dip of the strata of the foot wall of the fault, can be explained only as a result of the fault.

West Brookside Colliery.—Several transverse and one longitudinal fault have been developed in the workings of No. 4 slope.

The bottom of this slope is in the synclinal axis, which has been developed in the workings of No. 1 and No. 2 slopes.

In order to establish the east gangway on the north-dipping flexure, the top rock of the bed had to be stripped across the synclinal axis, by which "a transverse fault" has been crossed, *Fig. 12.* The fracture or fault dips about 70° south, is about two inches wide, and filled with disintegrated coal of a brownish color. The dislocated portion of the coal bed on the hanging wall of the fracture, is about ten inches lower than the portion of the bed on the foot wall of the fracture.

The east gangway on the south-dipping flexure has intersected a transverse fault, by which the portion of the bed on the hanging wall is about three feet lower than the portion of the bed on the foot wall of the fault, *Fig. 13.*

Both faults are of moderate extension, but exposed distinctly beyond any doubt as to their character.

A longitudinal fault (in the state of its origin) has been developed in breast No. 102 on the south-dipping flexure. There is no distinct fracture visible, but the bed is thicker than when sound and the coal in a fragmentary condition, notwithstanding that the dip of the bed is about 20°, *Fig. 14.*

A number of contractions of the bed have been crossed by the extensive workings, but they were not investigated during our visit.

Extensive longitudinal faults will be met with in the workings below the present deepest level of No. 3 slope, *Fig. 15.*

Williamstown Colliery.—Fig. 15 represents a sketch of the ground plan of Williamstown and Brookside Collieries, and *Fig. 16* a section through the tunnel of this colliery. The main Lykens Valley bed has been in an exceedingly good condition down to the present deepest level. The strata of the northern part of the tunnel has been intersected by numerous fractures, some of them from six to eighteen inches wide, and filled with disintegrated rock, which will prove to be principally longitudinal faults of great extent; for the thickness of the strata between Lykens Valley and Mammoth beds is about 956 feet in Eckert Colliery near Tremont (*Second Geological Survey of Pennsylvania*); and if the thickness of the strata does not increase towards the west or Williamstown, then the tunnel should have intersected the principal

leader of the formation, the Buck Mountain bed, and also the Mammoth. Instead of crossing these beds, numerous splinters of coal beds only have been intersected, and the Mammoth bed crops out a little north of the tunnel mouth. There is no reason for the supposition of an increase of the thickness of the strata between the coal beds referred to, west of Eckert Colliery, because the northern flexures of the synclinal, from the Mammoth to the lowest Lykens Valley beds, have been explored at Klingers Gap, about two miles east of the northern tunnel mouth, but I do not recollect the distance between them; however, from the junction of Bear and Rausch Creeks, near which the Mammoth bed outcrops, the distance to the lowest Lykens Valley bed and red shale will hardly be more than 1,000 feet. The dip of the strata is here about 60° south, or nearly the same as the dip of the strata in the most northern part of the tunnel, and the Mammoth bed north- and south dipping flexures. Therefore, the great distance between the Lykens Valley and Mammoth beds across the tunnel must have been produced by dislocation of longitudinal faults, which is demonstrated by facts. The faults will be developed in the workings of Williamstown and Brookside Collieries below the present deepest levels, sooner or later, according to their general dip and strike; but if the dip of the Lykens Valley bed decreases in the future deeper levels of Williamstown and Brookside Collieries, certainly it will be the "forerunner" of the approach to the synclinal elbow of a longitudinal fault.

Sections of the Second Geological Survey of Pennsylvania.—*Figs. 17* and *18* represent copies of Sections 2 and 3 of vol. i, of the Southern Coal Field, *Second Geological Survey of Pennsylvania.* According to its construction, the whole carboniferous formation has been pushed and tilted into one inversion and two sharp anticlinals, or into four synclinals and three anticlinals, inside of a horizontal distance of about 2,800 feet, without a single fracture.

The strata of this section of the coal field have been crossed by numerous adits, in which an unusually sudden replacement of the country rocks of the beds is demonstrated. For instance, the country rocks of the *F* or Primrose bed are transformed from sandstone into slate, and the reverse, within very short distances. A coal bed about eight feet thick, about 100 feet above the Primrose, has been squeezed out of existence in the so-called "Hell

Kitchen" overturned basin; it reappears in the next Greenwood basin and disappears again in Lansford basin.

Contractions of the Lykens Valley beds have been developed in Kalmia and Lincoln Collieries for very long distances, but no sudden and complete disappearance of the beds, nor sudden exchanges of the country rocks have been demonstrated; for, they are naturally deposited coal beds of small thickness.

The Holmes bed, eight feet of coal between *E* and *F* beds, has, as illustrated, doubled upon itself without fracture to a wedge-shaped large coal body in Rhume Run overturned anticlinal, which cannot take place, because the doubling upon itself would have to extend on the strike also for a certain distance and thus represent an instance as illustrated to occur in the great bed of Lorberry Creek, or an overlap in the bed itself with fracture (*First Geological Survey*, *i*, 237); consequently, if no overlap in the bed itself occurs without fracture, certainly overlapping of the whole strata cannot take place without fracture and more or less dislocation; for the latter conclusion is confirmed by the overlaps in Kohinoor, Middle Creek, Otto and Williamstown Collieries.

Based on these occurrences, I constructed from the Mine Sheets I and II, of vol. i, of the Southern Coal Field, *Second Geological Survey of Pennsylvania*, the sections, *Figs. 19*, *20*, *21* and *22*.

The Lehigh Coal and Navigation Company has lately sunk two shafts, and through them has been proved that the Greenwood basin and shaft anticlinal, as projected on the maps of the *Second Geological Survey*, are not in existence in that locality.

Figs. 23 and *24* are copies of maps of the *Industrial Statistics of Pennsylvania*, of 1885.

From *Fig. 23* it will be seen that the Primrose bed, or whatever its local name may be, has been inverted on its dip line for about 400 feet, without fracture.

Assuming the inversion exists, as illustrated, then the horizontal extension or strike of the bed must show a similar figure for a greater or less distance. Before inversion took place, all the strata have been in an undisturbed condition; consequently, the series of beds above and below the Primrose must have been tilted and folded into a very oblique inversion, and thus the lower Mammoth bed must have shown an unbroken inversion, too. A glance at the section will convince one that this perhaps is supposed to be

developed yet; but then, the cross cut passed regular south dips of the strata and then a pretty piece of coal (perhaps dirt), which is in a very pointed shape near the bottom of the cross cut. Where did this body of coal, indicated as a fragment of the splintered Mammoth bed, come from? It is obvious that it must be a fragment of the splintered Mammoth bed, and the splintering itself could be produced only by one or more fractures and dislocations.

Incidentally, the connection of the pointed coal body with the inverted leg of the Primrose bed forms a straight line, and there ought to be no doubt, that this line represents the fracture on which the strata on the foot wall of the fracture has slid down, thus producing a longitudinal fault or overlap, most probably through the whole carboniferous formation.

The dotted line, *D D*, indicates the bottom line of the Mammoth bed, constructed from the Mine Sheet III, *Second Geological Survey*, and no explanation is needed to prove that the Greenwood basin and the shaft anticlinal do not strike this location, as illustrated on the maps of the Survey.

Shaft No. 2, *Fig. 24*, passed from the surface down for about 150 feet on the anticlinal elbow of a longitudinal fault, or overlap, of a good-sized coal bed. (Strata of the hanging wall of the fracture.) From about 180 feet deep, south dips of the strata have been met, but the given data permit no conclusion, if, or where, a fracture has been struck. The dip of 55° has dropped to 26° within a very short distance, thus indicating a disturbance of the strata. The dotted lines, constructed from Mine Sheet II, *Second Geological Survey*, represent, approximately, the bottom lines of the Mammoth and Primrose beds. The latter fits well, provided the shaft is located about 250 feet southeast of the mouth of Tunnel No. 6. The dip of the strata through which the shaft passed, indicates that the existence of the Greenwood basin and the shaft anticlinal, as constructed on the maps of the *Second Geological Survey*, are entirely miscalculated, because all of the strata have been dislocated by one or several longitudinal faults of considerable extension.

Figs. 25 and *26* are copies of the Sections of the Map, "Part of the Mahanoy and Shenandoah basins, *Second Geological Survey of Pennsylvania.*

A probable "line of fault" is illustrated in section *C D, E F*, which is the *only one* supposed to exist in the anthracite region (at least according to the maps in possession of the University of Pennsylvania), and will be recognized, without any doubt, as a longitudinal fault or overlap. On the left or north side of this section, an overlap of about 500 feet on the dip, affecting the whole coal measures, is demonstrated. Now, if in the very section on the southern side "a line of faults" is necessary to explain the positions of two flexures of the Mammoth bed, why should there have been produced an overlap without fracture on the northern side?

Fig. 27 represents a section constructed from the corresponding Mine Sheet of the *Second Geological Survey*, on the theory hitherto advocated.

Fig. 25 represents a section about 11,600 feet east of section *C D*, in which the northerly overlap, already referred to, is constructed as a squeezing out of existence of considerable strata, which would be explainable only by the existence of "a dyke" The strata between Mammoth and Lykens Valley beds, in a thickness of about 740 feet, or 1,480 feet in all, have been squeezed to a thickness of about 200 feet within a distance of about 900 feet, which, if verified, would be a phenomenon heretofore not experienced in any part of the globe. I constructed section *Fig. 28*, from the corresponding Mine Sheet of the *Second Geological Survey;* but it is certain that the steep flexure, *a b*, of the Mammoth bed does not exist, because the gentle rollings of the bed, here two, have been developed to a greater number in Kohinoor Colliery, about 8,400 feet westwards, and it will be proved by the workings (if not developed already) that they exist in nearly the same number in Knickerbocker Colliery also.

Figs. 29 and *30* represent sections of the ground between the sections of *Figs. 25* and *26* of the *Second Geological Survey*, or of sections, *Figs. 27* and *28*, constructed by me, which need no further explanation than a comparison with my arguments on the matter.

It is a fact, that each mining engineer of the anthracite region has experience of some occurrences of overlaps, which proves the numerous occurrences of overlaps and the importance attached to them, and, furthermore, it will not be questioned that numerous

collieries have suffered by the abrupt termination of their coal beds, caused by longitudinal faults; and, therefore, the prevalent definition of longitudinal faults is most surprising. For instance, on p. 236, *First Geological Survey of Pennsylvania*, it is stated:

"The coal has rubbed upon itself and is in a more or less fragmentary, friable and polished state."

The polished state of the coal referred to, hypothetically admits as a fact, that the coal, and consequently the rocks also, at the time of their disturbance have been as hard as they are to-day; for, fragments of soft coal and rocks will never polish each other, and, therefore, a doubling upon itself of the roof, or the floor, or of both, as referred to, cannot take place without fracture and more or less dislocation. The doubling upon itself is also impossible; because the measures consist of a number of strata, of which *not one* can be shown to be inverted.

The existence of parallel displacements, or parallel, slips to an extraordinary degree in the coal beds of the Sharp Mountain (which, of course, must be accompanied by displacements on the strike also), has been reported by the *First Geological Survey* with the statement, that, if these parallel slips do not take place by the beds doubling upon themselves, they must have arisen from a fracture, *Fig. 2A.*

In the *Report of the First Geological Survey*, already referred to, is stated:

"It may be accepted as a general rule, not without its occasional exceptions, that these longitudinal faults consist of an up-throw of the vein to the S (South?) of the fault, so that the remainder of the vein, if we are mining downward, will be recovered at a point above and further back towards the outcrop, compared with the spot, where the rock wall crosses the mine."

Such a fault of considerable extent is illustrated by the section of the Kittatinny Valley (*First Geological Survey of Pennsylvania*), to occur in the blue slates near Wormleysburg, but none has been reported to occur in the anthracite region.

A comparison of the fault near Wormleysburg with the occurrences of longitudinal faults, developed in anthracite mining, a few of which are described before, and with the faults constructed on sections, *Figs. 19, 20, 21, 22, 27, 28, 29* and *30*, will show their great similarity, leaving no doubt at all, that overlaps have been

produced by dislocation on a fracture. But still, as many geological matters are enveloped in mystery yet, a short, inverted leg of an "*overturn*" in a coal bed may be found yet, although no such a thing, whether in the old extensive coal mines of Europe, or in anthracite mining in Pennsylvania has been developed thus far.

A comparison of the general rules applicable to faults, based on a long experience in mining in Europe will also show, that there is no room for the notion of the existence of a short, inverted leg of an overturn in a coal bed.

The origin of transverse faults in ore veins has been defined as in consequence of a sinking of the rocks on the hanging wall of the fracture or fault by Schmidt, Frankfurt, 1810; and this theory has been confirmed by numerous occurrences met with in "ore" and "coal" mining. (Zimmermann, Leipzig, 1828; *Engineering and Mining Journal*, of New York, vol. xxxiv, p. 56.) The rule to apply to such a fault is:

If a horizontal mine work strikes a fault dipping towards, cross the fracture and turn parallel to it towards the hanging wall; if the fault dips off, cross it and turn parallel towards the foot wall.

The origin of longitudinal faults has been defined by Heim, *Mechanism of the Strata*, Basel, 1878, and by G. Koehler, Professor of Mining at Clausthal, to be in consequence of disproportionate sinking, of different portions of the strata, thus causing first gentle synclinals and anticlinals, which became more or less uplifted, and finally the flexures were fractured and more or less displaced by prodigious forces, continuously acting.

The course of the fracture indicates to a certain degree the limits of disproportionate forces, and generally ranges nearly with the strike of the flexure and strata.

The definition stated above justifies the conclusion:

Whatever the course of the fault may be, the sliding down on the fracture of one portion of the strata, or even the disproportionate sliding of both portions, necessarily must be of two different characters.

If, for instance, a coal bed, *a b*, *Fig. 1*, is disconnected by a fault, *c d*, and either the portion *a* on the hanging wall of the fault, or the portion *b* on the foot wall of the fault, or even both slide down disproportionately, according to the height of sliding from their natural position, their appearance will be either as illustrated in *Fig. 1*, or as illustrated in *Fig. 2*.

In *Fig. 2*, the portion *a* on the hanging wall of the fault, is in a higher position, at the same time producing an overlapping of the measures, or *a longitudinal fault.*

In *Fig. 1*, the portion *a* on the hanging wall of the fault is in a lower position, and this is *a transverse fault.*

Dislocations, as illustrated in *Fig. 2*, are possible only when the strike of the fault ranges with the strike of the axis and strata, and the inclination of the fault, greater than the dip of the strata, is always of the course of the dip of the strata. They occur mostly in the neighborhood of anticlinals and synclinals, and have the same origin. Such faults are designated by the authorities referred to (Heim, Koehler) as the highest degree of folding, illustrated in *Fig. 31*, and it is evident that the portion on the foot wall of the fault has slid down. This explains that the cutting out of coal beds by longitudinal faults are generally preceded by contractions, and a bending towards the fault on the dip, as well as on the strike. The downward bending of the strata on the hanging wall of the fault (anticlinal elbow, *First Geological Survey*) may have been produced by the enormous weight of the overlying strata, and the prodigious oblique force of the sliding strata on the foot wall of the fault, while the up-bending (synclinal elbow, *First Geological Survey*) of the strata on the foot wall of the fault, may be explained by a certain resistance of the underlying strata, which necessarily must have produced an oblique movement of the strata in the direction of its strike with similar results. The rule to apply to a longitudinal fault is:

If a horizontal mine work strikes a fault dipping towards, cross towards the foot wall or bottom strata, if the fault dips off, cross towards the hanging wall.

Dislocations, as illustrated in *Fig. 1*, occur mostly with a strike of the fault, transverse to the strike of the axis and strata, they often cross anticlinals and synclinals, and therefore they are considered generally to be of later origin than the synclinals and anticlinals crossed by them. The portion of a vein or of a coal bed on the hanging wall of the fault has always slid downwards, even if the dip of the fault is in an opposite direction from the strata, as illustrated in *Fig. 32*.

The Maps of the *Second Geological Survey of Pennsylvania*, to a certain degree, are reconstructions of the mine maps; therefore,

the illustrations of overlaps without fractures, one single instance excepted, force one to the conclusion that either the mine maps do not represent the condition of the strata, or the survey has failed to realize its anticipations.

A SIMPLIFICATION OF THE NEW WEATHER SIGNAL CODE.

BY PROF. EDWIN J. HOUSTON.

[*Abstract of Remarks made at the Stated Meeting of the* INSTITUTE *held Wednesday, May 18, 1887.*]

In the new code of weather-signal flags recently adopted by the United States Weather Bureau, approaching changes in the weather, as is well known, are indicated as follows, viz.:

The triangular black temperature flag indicates, by its position above or below the clear weather (a white square flag) or the precipitation flag (a blue square flag), a higher or lower temperature, respectively; thus, *Fig. 1*, indicates clear weather, higher temperature; while *Fig. 2* clear weather, lower temperature; *Fig. 3* rain or snow, with higher temperature; *Fig. 4* rain or snow, with lower temperature. The cold-wave flag, *Fig. 5*, a square black

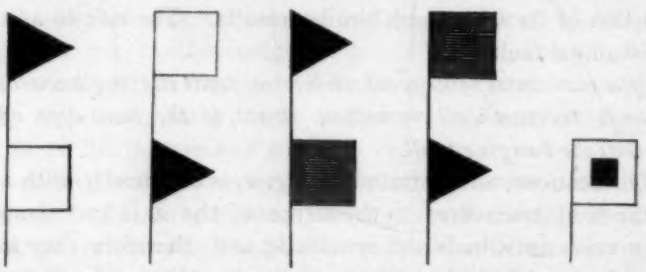


FIG 1. FIG. 2. FIG. 3. FIG. 4. FIG. 5.

centre on a white ground, is displayed whenever a decided fall in temperature is indicated. It is usually displayed at least twenty-four hours before the fall in temperature is expected to come.

The cold-wave flag, of course, is never displayed along with the temperature flag. The other three flags, however, may be dis-

played simultaneously. In this case, when displayed from poles, the indications of the upper flag are to be taken first. When displayed horizontally (see *Fig. 6*), the left-hand flag is to be taken first, indicating rain or snow, followed by fair but colder weather.

FIG. 6.

The author proposes a simple modification of the above code, which, while it will do away with the cold-wave flag entirely, will, nevertheless, permit the indications both of a cold and a hot-wave when it is so desired.

This modification is, briefly, as follows: while the triangular temperature flag is employed as before to indicate warmer or colder weather, respectively, according to its position above or below the clear weather, or the precipitation flag, the extent of the change towards heat or cold is indicated by the distance it is placed above or below these flags.

According to the code as now employed, the separate flags are displayed separated from one another a distance equal to once their diameter. In the code as modified, it is proposed to do away with the cold-wave flag and replace it by the triangular temperature flag displayed below the clear weather or the precipitation flag, a distance equal to at least twice the diameter of any of the separate flags. Thus, *Fig. 7* indicates clear weather; cold wave.

FIG. 7.

FIG. 8.

FIG. 9.

FIG. 10.

In the same way the triangular temperature flags, when displayed

above the other flags, at a distance equal to at least twice the diameter of a single flag, indicates an approaching hot wave. Thus, *Fig. 8* indicates clear weather; hot wave.

Since the poles or horizontal supports for the signal flags, as now employed, provide sufficient space for the simultaneous display of three flags, no difficulty will be experienced in obtaining the distance of twice the diameter of a single flag as required by the proposed modification.

No difficulty need be anticipated in distinguishing between the lower temperature indicated, and that of a cold wave as indicated in the two flags (*Figs. 9, 10*), since whatever the distance of the observer, the apparent size of the flags will always readily permit a ready decision as to whether they are at a greater distance apart than once their diameter.

It is evident that, when so desired, the severity of the hot or the cold wave can be indicated by displaying the temperature flag a distance above or below the other flags greater than twice their diameter. Or, should such refinement become desirable, distances equal or proportional to the diameter of a single flag might be marked off on the support, to indicate differences of 5°, 10°, or 20°.

The author believes that the excellent code now in use would be greatly improved and simplified by the modifications he proposes.

CENTRAL HIGH SCHOOL,
Philadelphia, June 24, 1887.

NOTE ON LONG COLUMNS.

By WM. CAIN, Prof, Math. and Eng., S. C. Mil. Academy.

(*Concluded from page 61.*)

ANOTHER FORMULA, INVOLVING THE BREAKING STRESS OF THE MATERIAL.

To point out the difficulties of the subject, it may be well to deduce a formula that shall include the breaking stress of the material, and that will lead, on certain suppositions, to the well-known Rankine form, whose insufficiency can thus more clearly be realized.

As in practice there is nearly always some eccentricity in the position of the load, whose influence is very marked, particularly in the case of short columns, where theoretically there can be no bending except for this eccentricity, we shall include it from the start in the formula.

Thus, in the figure, let the load, P, act at a distance, k, to the left of the centre line of the column, $A\,B$, which, after strain, bends to the curve, $A'\,D\,B$; then calling, as before, $f = C\,D$ the deflection at the centre from the axis, the moment of P with respect to D will now be $P(f+k)$. We shall further designate by Q the area of the cross section at D, by z the distance from the centre of gravity of the section to the most compressed fibre, and by E, I and r the modulus, moment of inertia and radius of gyration of the section about a horizontal axis through D, perpendicular to the plane of the paper.

Now, if we conceive applied at D, the centre of gravity of the cross section, two opposed forces, both equal to and parallel to P, then the downward force at D causes a uniformly distributed stress over the section Q of intensity,

$$p_1 = \frac{P}{Q};$$

and the upward force at D with the load P, k inches to the left of A, form a couple, whose moment is $P(f+k)$, which causes a

uniformly varying stress in the section, whose greatest intensity, at the most compressed edge, is

$$p_2 = \frac{P(f+k)z}{I}$$

The greatest compression per square unit at the most compressed edge of the section at D is, therefore,

$$p_0 = p_1 + p_2 = \frac{P}{Q} + \frac{P(f+k)z}{I}$$

in which expression, if P represents the breaking load, p_0 represents the ultimate compressive unit stress, supposing, approximately, the law "*ut tensio sic vis*," holds up to the failing point.

Putting $I = Q r^2$, we can write the above formula,

$$p_0 = \frac{P}{Q}\left(1 + \frac{(f+k)z}{r^2}\right) \qquad (10)$$

Now we shall introduce *our first approximation* by supposing the curve $A'\,D\,B$ to be *an arc of a circle* of radius ρ, so that we have, calling l the length of the column,

$$f(2\rho - f) = l^2 \quad \therefore \rho = \frac{l^2}{2f} \text{ nearly.}$$

The last relation is sufficiently true for flat arcs, and is exactly true for the radius of curvature at D, if we regard the arc $A'\,D\,B$ as parabolic.

Next let us consider two cross sections after strain at D, ds apart along the axis, then if a = change in length of a fibre parallel to the axis, one unit long and at the distance z from the axis of the column, due to the bending moment alone, we have the well-known relation,

$$\rho : ds :: z : a\,ds \qquad \therefore \rho = \frac{z}{a}.$$

Equating this value of ρ with the previous one, and noting that $a = \frac{p_2}{E}$, we have

$$\frac{z}{a} = \frac{l^2}{2f}$$

$$\therefore f = \frac{l^2 a}{2z} = \frac{l^2 p_2}{2Ez}; \qquad (11)$$

which, substituted in the value for p above, gives

$$\frac{P}{Q} = \frac{p_0}{\left(1 + \frac{k z}{r^2}\right) + \frac{p_2}{2E}\left(\frac{l}{r}\right)^2} \qquad (12)$$

This formula, within the limits of elasticity, is perfectly correct, provided the elastic curve is taken approximately as a circle or parabola; but as we know neither k nor p_2, we may introduce errors just at this point by regarding them both as constant for all length ratios; for although k may be nearly constant for the same shapes and end connections and care in fitting, yet $p_2 = \frac{P(f+k)z}{I}$ varies with f and probably is not constant for all length ratios. Placing $\left(1 + \frac{k z}{r^2}\right) = a$, and $\frac{p_2}{2E} = b$, (12), can be written,

$$\frac{P}{Q} = \frac{p_0}{a + b\left(\frac{l}{r}\right)^2} \qquad (13)$$

In the well known Gordon-Rankine formula the load is not supposed eccentrically placed, so that $k = o$ and $a = 1$; also p_2 is regarded as constant (which is doubtless erroneous) and b is treated as a constant to be determined by experiment.

The writer first pointed out in *Van Nostrand's Magazine*, for November, 1877 (in criticising the Hatzel formula), that b was not necessarily a constant, though it is remarkable how near the results agree in a general way with experiments at the breaking limit for values of $\frac{l}{r}$ varying from 30 to 600, upon the supposition that b is a constant. It is much more rational though not to assume the eccentricity of the load zero, but determine, from the average of the great number of experiments that have been made, an average value for a, and likewise ascertain whether b can be treated practically as a constant or otherwise (see *Weisbach's Mechanics*, vol. i, p. 545, for a similar determination). Now, a difficulty presents itself at once in pursuing this plan, namely, the determination of p_0 for wrought iron, for its value has been given all the way from 36,000 to 60,000 pounds.

I believe the value 36,000, given in Gordon's formula, to be

erroneous, since Fairbairn and others have shown by experiments on transverse breaking of wrought-iron beams, properly designed at the compression flange, so as to give way there, if at all, by direct crushing, and not by bending, that the tensile unit stress (exerted at the lower flange) was practically equal to the compressive unit stress exerted in the top flange. If the tensile unit strength of the iron was, say, 55,000, then we should place $p_0 = 55{,}000$ in the formula. In fact, I find that for $p_0 = 56{,}000$, $a = 1{\cdot}4$ and $b = \frac{1}{20000}$ for wrought-iron columns, with fixed ends, that the formula strikes an excellent average of the results given on Mr. Wilson's diagram (see *Trans. Am. Soc. Civ. Engs.*, vol. xv,) for length ratios varying from 0 up to 240. For higher length ratios, b should gradually increase from $\frac{1}{20000}$ to $\frac{1}{18000}$ (see Mr. Johnson's paper, mentioned further on). The formula is very flexible; for not knowing p_0, a or b, we can determine them to suit the results between any limits, though of course *the formula becomes largely empirical by so doing.* Thus the values $p_0 = 50{,}000$, $a = 1{\cdot}2$ and $b = \frac{1}{20000}$ for *wrought-iron* columns with *fixed ends* will agree fairly with experiments from $\frac{l}{r} = 0$ to $\frac{l}{r} = 500$, though the agreement for the smaller length ratios is not so good as on the first assumption. Similarly the values $p_0 = 50{,}000$, $a = 1{\cdot}1$ and $b = \frac{1}{10000}$ will approximate to the average of the experiments on *wrought-iron columns, hinged ends,** though not nearly so well for $\frac{l}{r} < 240$ as for higher length ratios where a good average is struck.

For *cast-iron* columns with *fixed ends*, the values assumed were $p_0 = 100{,}000$, $a = 1{\cdot}2$, and $b = \frac{1}{5000}$, which gave excellent results for all length ratios.

Some of the principal results are recorded in the following table:

Wrought-Iron Columns, Fixed Ends.

$\frac{l}{r}$	0	50	100	150	200	300	400	500
$\frac{P}{Q}$	41667	37700	29400	21500	15600	8770	5430	3650

* The eccentricity of the load in the case of "hinged ends" is only realized when friction between the pin and column is exerted, and its *extreme limit* is easily found, as in the case of trunnions, by known laws of mechanics.

Wrought-Iron Columns, Hinged Ends.

$\frac{P}{Q}$	41667	37000	23800	15000	9800	4950	2900	1900

Cast-Iron Columns, Fixed Ends.

$\frac{P}{Q}$	83333	58800	31300	17600	10900	5200	3000	2000

For $a = 1{\cdot}2$, as found for fixed ends, we have $k = 0{\cdot}2\,\frac{r^2}{z}$, from which we can determine the eccentricity of the load for an assumed cross section. Thus, on substituting the well-known values of r, the radius of gyration, and z, the distance from the axis to the most compressed edge of the column, we find that for *rectangular sections*, $k = \frac{1}{30}$ depth; for *solid cylindrical* columns, $k = \frac{1}{40}$ diameter; for *thin, hollow* cylinders, $k = \frac{1}{20}$ diameter; for thin, hollow, square columns, $k = \frac{1}{15}$ depth; and for *common columns*, formed of two channels connected by latticing, which give way in the direction of the latticing, $k = \frac{1}{10}$ depth of latticing. Although the eccentricity is thus found to be small, it can be objected that the formula gives different eccentricities to different shapes whose ratio is seemingly arbitrary, for which there can be no good reason. A separate formula, then, for each shape would alone answer the objection. The same objection holds, however, with respect to any *one* formula that may be devised for all shapes, and is not very serious when a rough average of the whole is alone desired.

For the hinged ends, where $a = 1{\cdot}1$, k has one-half the values given above. In cases where the direction of P is not parallel to the axis, or even crosses it, k would still represent the eccentricity at the dangerous section.

We note that, for wrought iron, as $\left(\frac{l}{r}\right)$ approaches the limit zero, that the *average* intensity of stress on the cross section $\frac{P}{Q}$ approaches the value 41,667 pounds, which is only a fractional part of the assumed greatest intensity, 50,000 pounds, as it should be. Similarly for cast iron.

The form proposed would, therefore, seem to be far preferable to the ordinary Gordon form in this respect, though both labor under the objection of being semi-empirical in character.

It may then be inquired whether a simple, purely empirical formula cannot be devised from the experiments that will give better results than the Gordon and allied forms?

In the discussion on Mr. Wilson's paper, before mentioned, there were certain empirical formulæ proposed, as embodying the average results of experiments on wrought iron, by E. Thacher, M.Am.Soc.C.E., which, within the limits, $\frac{l}{r} = 20$ to 200, are found to give as good or better results than more complicated formalæ.

This whole subject, however, has been so fully presented by Thomas H. Johnson, M.Am.Soc.C.E., in a paper "On the Strength of Columns," read at the Annual Convention of the American Society of Civil Engineers, June 26, 1885, that one has but to glance at the admirable diagrams given in that paper, to see that the kind of formulæ proposed by Mr. Johnson represent about as nearly as is possible for any set of formulæ to represent, the average of the great number of experiments that have been made on the breaking weights of columns.

In the numerous diagrams given, the abscissas represent the ratio of the length to the least radius of gyration $\left(\frac{l}{r}\right)$ and the ordinates, the corresponding average breaking stress $\frac{P}{Q}$ on the cross section.

Mr. Johnson found for all materials that "(1), that part of the line corresponding to the higher length ratios, is a curve, the equation of which is Euler's formula; (2), that part of the line corresponding to the lower length ratios, is a straight line tangent to the aforesaid curve, and intersecting the vertical axis at a point which is constant for each material in all the varieties of end bearings; (3), the ordinate at the point of tangency is one-third of that part of the vertical axis intercepted by the tangent."

The second law above is in agreement with the proposed formulæ of Mr. Thacher for wrought iron, as far as expressing the average breaking stress as a linear function of the length ratios is concerned. Mr. Johnson has drawn curves for maximum and minimum, as well as average values, and expressed likewise by the formulæ given below, for various materials, the average stress $\frac{P}{Q}$

on the cross section at the breaking limit for various ratios of l to r.

For the sake of brevity, let us call,

$$p = \frac{P}{Q} = \frac{\textit{breaking load in pounds.}}{\textit{cross section in inches.}}$$

$$x = \frac{l}{r} = \frac{\textit{length of column in inches.}}{\textit{radius of gyration of cross section in inches.}}$$

$$x' = \left(\frac{l}{r}\right) = \textit{abscissa of point of tangency below which the equation of the tangent is used and for greater values of } x \textit{ the curve of Euler's equation.}$$

We give in the table below the special forms of the equations for various materials and end bearings, observing that the experiments on oak and oolitic limestone are comparatively meagre; but it is understood in formulæ of this kind that the constants are to be amended from time to time as experience dictates. For wrought iron, the formulæ embrace the average results of a very large number of experiments for every style of cross section and extending up to 500 radii of gyration.

On looking over these formulæ, which largely embody the results of experiments, the question naturally occurs, Why does Euler's formula give the crippling load for very long columns, but not for the shorter ones? The answer is, in a general way, that in the first case the limit of elasticity has not been exceeded when bending just occurs; in the last case it has been. Thus, for columns hinged or round at both ends, we have for $f = o$ from (7), since the quantity in brackets is equal to zero, and making $i = 1$,

$$\frac{2a}{r} = \frac{\pi\sqrt{\frac{e}{P}}}{\sqrt{1-\frac{P}{e}}} = \pi\sqrt{\frac{e}{P}} \text{ nearly,}$$

in which $\frac{P}{e} = \frac{P}{EQ}$ = shortening of column per unit of length. For *wrought iron*, take $E = 27{,}000{,}000$, and *at the elastic limit* the unit stress, $\frac{P}{Q} = 27{,}000$, which, in the above formula, gives $\frac{2a}{r} = 100$ nearly. Now, if we suppose all the terms of the

Materials.	End Bearings.	Equation of Tangent.	$x' = \frac{l}{r}$	Equation of Curve.
Wrought Iron. $E = 27{,}000{,}000$	Flat,	$p = 42000 - 128\,x$	218·1	$p = \frac{666{,}090{,}000}{x^2}$
	Hinged,	$p = 42000 - 157\,x$	178·1	$p = \frac{444{,}150{,}000}{x^2}$
	Round,	$p = 42000 - 203\,x$	138·0	$p = \frac{266{,}490{,}000}{x^2}$
Mild Steel. (Carbon = 0·12.) $E = 27{,}000{,}000$	Flat,	$x = 52500 - 179\,x$	195·1	$p = \frac{666{,}090{,}000}{x^2}$
	Hinged,	$x = 52500 - 220\,x$	159·3	$p = \frac{444{,}150{,}000}{x^2}$
	Round,	$p = 52500 - 284\,x$	123·3	$p = \frac{266{,}490{,}000}{x^2}$
Hard Steel. (Carbon = 0·36.) $E = 27{,}000{,}000$	Flat,	$x = 80000 - 337\,x$	158·0	$p = \frac{666{,}090{,}000}{x^2}$
	Hinged,	$p = 80000 - 414\,x$	129·0	$p = \frac{444{,}150{,}000}{x^2}$
	Round,	$p = 80000 - 534\,x$	99·9	$p = \frac{266{,}490{,}000}{x^2}$
Cast Iron. $E = 16{,}000{,}000$	Flat,	$p = 80000 - 438\,x$	121·6	$p = \frac{394{,}720{,}000}{x^2}$
	Hinged,	$p = 80000 - 537\,x$	99·3	$p = \frac{263{,}200{,}000}{x^2}$
	Round.	$p = 80000 - 693\,x$	77·0	$p = \frac{157{,}920{,}000}{x^2}$
Oak. $E = 1{,}200{,}000$	Flat.	$p = 5400 - 28\,x$	128·1	$p = \frac{29{,}604{,}000}{x^2}$
Oolitic Limestone. $E = 4{,}350{,}000$	Flat,	$p = 9000 - 32\,x$	189·1	$p = \frac{107{,}314{,}500}{x^2}$

formula to remain constant but the length of column $2a$, we see that an increase of length in formula (7) will give some deflection for the same P, so that bending will *just* begin for a *less* uniform compressive stress than the 27,000 assumed; therefore, when the column begins to fail by bending for $\frac{2a}{r} > 100$, the limit of elasticity has not been passed, at the instant when bending begins, so that Euler's formula should exactly apply, provided all the theoretical hypotheses (of a straight column of homogeneous material, with a load acting along the axis and $i = 1$) are exactly realized. The reverse holds for shorter columns; thus, for $\frac{2a}{r} = 77$, we find that $\frac{P}{Q} = 45{,}000$ pounds, or the elastic limit has been long exceeded before the column, theoretically, begins to bend at all. If 45,000 pounds was the crushing unit stress, we see for this ratio, $\frac{2a}{r} = 77$, and for all lower length ratios that the column should, theoretically, give way by crushing entirely, of uniform intensity over the whole cross section, as no bending is possible if the original hypotheses are realized.

In a similar manner, we find the least values of $\frac{2a}{r}$ for which the elastic limit has not been passed when bending is just about to begin, and Euler's formula becomes applicable—for *mild steel*, 87; *hard steel*, 67; *cast iron*, 70, using the moduli given in the table above, and regarding the elastic limits in order: 35,000, 60,000 and 32,000 pounds.

For columns with *fixed ends*, these ratios are all doubled, as we easily deduce from the formulæ corresponding to fixed ends, given above.

These results show, too, in a general way, that the inapplicability of Euler's formulæ to usual column lengths, as shown in the table above, is not entirely, or even principally, due to "the innate perversity of inanimate things," but rather that the hypotheses of perfect elasticity, etc., upon which the theory is founded, no longer, even approximately, holds.

The empirical formulæ proposed answer all the needs of practice, though it is possible that formula (13) may answer better for

any one shape; but where a general average only is desired, the linear equations above cover nearly all practical cases, and they are, of course, preferable to Euler's formulæ, which give values entirely too large for small length ratios; also, to the Gordon-Rankine form, not only on account of their simplicity, but because for very small length ratios they are more accurate, and for medium length ratios equally as accurate, as the partly empirical formulæ they are intended to replace; on which accounts the above empirical formulæ are cordially recommended for practice.

APPENDIX.

In *Executive Document* 12, 47th Congress, 1st Session, is given the Watertown experiments on posts of white and yellow pine, varying in size from about 5·2 inches square to a cross section of 8·5 x 16·5 inches and up to 62 diameters in length. (Also, see Lanza's *Applied Mechanics*, pp. 511–519.)

On plotting the ascertained unit stresses corresponding to given ratios of length to least side of the rectangular cross section $\left(\frac{l}{d}\right)$, it is found that the *average* unit stresses $\frac{P}{A}$ are approximately given by the following formulæ up to $l = 60\,d$ nearly:

$$\text{for } \textit{white pine}, \quad \frac{P}{A} = 3000 - \frac{100}{3}\left(\frac{l}{d}\right), \text{ and}$$

$$\text{for } \textit{yellow pine}, \quad \frac{P}{A} = 5500 - \frac{200}{3}\left(\frac{l}{d}\right),$$

in pounds per square inch of cross section.

The *minimum* unit stresses are about 400 pounds less for corresponding values of $\frac{l}{d}$ with but few exceptions. There were sixty-six experiments on white pine single sticks (which alone were considered), and seventy-two tests of the yellow pine posts.

It is hardly probable that in practice the end fittings are so perfect as in the Watertown tests, so that, although the above formulæ probably give us the average of the most complete tests that have yet been made on pine posts, yet a certain discretion must be used in applying them.

According to Mr. Johnson's rule, it will be found that for *white pine*, Euler's formula would have to be used for $\left(\frac{l}{d}\right)$ greater than 60, for which length ratios,

$$\frac{P}{A} = \frac{3{,}600{,}000}{\left(\frac{l}{d}\right)^2};$$

and for yellow pine the limit is $\frac{l}{d} = 55$, beyond which,

$$\frac{P}{A} = \frac{5{,}544{,}825}{\left(\frac{l}{d}\right)^2}.$$

But it must be carefully borne in mind that no experiments can be appealed to to substantiate these last values, which are much higher than sometimes given.

It should be noted that for "round" ends, Mr. Johnson used Euler's exact formula in the table above, but the experiments on iron showed that for flat ends, the coefficient $4\pi^2 = 39{\cdot}48$ of Euler should be changed to 24·67, which is probably on account of the unknown eccentricity of the load.

The FUTURE WATER SUPPLY of PHILADELPHIA.

In Chief Ogden's report of the operations of the Water Department of the City of Philadelphia, for the year 1886, which was submitted to Councils, on Thursday, July 14th, is given the final report of Rudolph Hering, engineer in charge of the "Surveys for the Future Water Supply." These investigations were begun in May, 1883, and continued during the years 1884, 1885 and until July, 1886. There was expended for the surveys $81,547.96.

In the course of his recapitulation of "conclusions that have been arrived at from the examinations," Mr. Hering says:

"In making these investigations, it has been taken for granted from the outset that the water from any point in the Schuylkill River, and from any point in the Delaware River, below Trenton, will not be of a sufficient good quality to furnish a future supply for the city, although the fact has been admitted that, at present,

the Delaware water at Lardner's Point, within the city limits, is not only fairly good, but is likely to remain so for some time.

"In looking about for an improved supply, every practicable scheme was considered. No success could be expected from a supply by artesian or driven wells in this locality, nor would filtering or purifying the water of the Schuylkill or lower Delaware give permanent satisfaction. The only schemes worth investigating were those which bring to the city the water of running streams in the Schuylkill, Delaware or Lehigh water-sheds.

"It required but little thought to see that the water from the streams north of the Blue Mountains would be the best available in quality not only now, but for an indefinite future, and that this region would therefore have to be the ultimate source of water supply for Philadelphia, and probably also for other cities lying between the mountains and the seaboard.

"To obtain an intelligent opinion on the cost of such a supply, surveys and examinations were made, which showed that inasmuch as water of good quality can be secured at a less expense from nearer localities, it is not advisable at once to go to the Blue Mountains.

"In adopting a scheme for an earlier future, this ultimate source, however, should be considered, so that the aqueducts now constructed could be available for the final source of supply. The quantity of water which it was thought best to calculate for at present was at least 200,000,000 gallons per day, or more than double the present consumption. The elevation at which the water should be delivered was fixed at about 170 feet above datum (the height of the present basin at Wentz's farm and the proposed basin at Cambria), because it gives the most favorable distribution for the city.

"The streams offering a good water supply nearer than the Blue Mountains are the Perkiomen Creek, a tributary of the Schuylkill River, the Tohickon and Neshaminy Creeks, tributaries of the Delaware River, and the Delaware River itself, above Trenton. In point of quality the water of the latter has been found to be the best; that of the Upper Perkiomen and Tohickon Creeks comes next in quality; and that of the Neshaminy and Lower Perkiomen Creeks is least good.

"An estimate of the cost of obtaining Delaware water alone

indicates that above Lardner's Point the most economical scheme is to bring it from Point Pleasant, because the river has quite a descent near this place, which materially reduces the height of pumping as compared with points lower down the river, such as Lumberville, New Hope and Yardleyville. Another advantage gained by this sudden descent is the water-power, which can be developed to furnish a daily supply of 120,000,000 gallons during the dry season.

"The cost of aqueduct, pumping plant and capitalized cost of pumping, amount to $19,622,543, if 210,000,000 gallons of water daily are pumped by steam, and to $15,475,262 if only 120,000,000 gallons are pumped by water and the remainder by steam.

"Purely gravity supplies, without pumping, can be obtained from either the Perkiomen Creek or from the Tohickon and Neshaminy Creeks combined. The latter project cannot be made to furnish a daily supply of over 156,000,000 gallons in years of minimum rainfall. While the water furnished by the Tohickon and Upper Perkiomen Creeks is good, that which is taken from the Neshaminy and Lower Perkiomen will be of much inferior quality. Neither of these purely gravity schemes would therefore be quite satisfactory.

"The cost of procuring a supply from the Perkiomen Creek is $13,674,493, and from the Tohickon and Neshaminy Creeks together, $13,846,662.

"Finally, a combined gravity and pumping scheme is possible by procuring water from the Tohickon Creek and from the Delaware River at Point Pleasant. The former can furnish on the average between 90,000,000 and 100,000,000 gallons per day by gravity; in minimum years only 80,000,000 gallons can be depended upon. The Delaware River, as we have seen, can furnish 120,000,000 gallons by water-power. Both the Tohickon and Delaware waters have been found to be not only of good quality, but much better than the waters of the Neshaminy, and particularly of the Lower Perkiomen Creeks. The cost of this scheme is $12,695,941, if the water-power is utilized, and $17,717,025, if steam-power is used.

"It is, therefore, clear that the best and most economical project to supply the city of Philadelphia with water is to bring to it the Tohickon water by gravity and to pump from the Delaware River at Point Pleasant by water-power.

"In order to perceive the relative values of the different schemes with still more distinctness, I have made three estimates; one, for completely filling the aqueduct; one, for furnishing 150,000,000 gallons, and one for only 90,000,000 gallons per day.

"To supply the latter quantity of water from the Perkiomen Creek requires an expenditure of $10,495,000. In bringing 90,000,000 gallons daily from the Delaware water-shed, it is found that the Neshaminy Creek alone could furnish the amount, except during years of minimum rainfall, at a total expense of $7,875,000. The Tohickon Creek also could furnish a quantity up to 90,000,000 gallons, except during very dry years, at a cost of $10,008,000. If the Delaware water at Point Pleasant is used, the cost for 90,000,000 gallons is $12,775,000, if pumped by steam, and $9,673,000, if pumped by water-power. At Lardner's Point, the cost would be $7,064,000.

"Therefore, to supply the city with 90,000,000 gallons daily of good water, which is the present consumption, the cheapest project is to pump the Delaware water at Lardner's Point; the next is the Neshaminy scheme, and the third is pumping Delaware water at Point Pleasant.

"To increase the supply to 150,000,000 gallons requires a total expenditure of about $12,139,000, if the Perkiomen water only is used, and a total expenditure of about $17,635,000, if no water is taken below Green Lane, and the deficiency supplied from the eastern affluents of the Lehigh River above the Lehigh Gap.

"On the Delaware areas the water stored from the Neshaminy and Tohickon Creeks together could furnish an amount up to 156,000,000 gallons at a cost of $13,846,662. If, instead of using the Neshaminy water, Delaware water is pumped at Point Pleasant, the cost would be $14,275,000, if steam, and $11,215,000 if water-power is employed. To supply Delaware water only would cost, if pumped by steam at Point Pleasant, $16,355,000, and at Lardner's Point, $10,415,000.

"For supplying 150,000,000 gallons daily, therefore, from beyond Lardner's Point, the project contemplating the use both of the Tohickon and Delaware water at Point Pleasant, pumping the latter by water-power, is the least expensive one.

"Finally, to increase the supply to 210,000,000 gallons, the Point Pleasant scheme, as already stated, is again the most economical one, besides furnishing decidedly the best quality of water.

"It, therefore, appears with sufficient clearness, I think, that whenever good water can no longer be obtained from Lardner's Point by the pumps which it may be considered advisable to place at this point, the city should build an aqueduct to Point Pleasant, pump Delaware water by water-power, and supplement the quantity as it may become necessary, by storing the water from the Tohickon Creek, first in the lower, and then in the upper reservoir.

"After the aqueduct is taxed to its full capacity, at which time it will probably be necessary to go to the Blue Mountains for an increased supply, another aqueduct will have to be built. It is premature, I think, to say definitely at present whether this second aqueduct extending to the Blue Mountains should go by way of the Delaware or the Lehigh River. If the South Mountain region should preserve its present character, there can be no doubt that it should extend by way of the Perkiomen Valley, and, after receiving the South Mountain water at Green Lane, follow up the Lehigh River. The cost of this scheme, which now is relatively greater than that of others, would then probably be less. The Point Pleasant aqueduct could later also be carried to the mountains whenever the quality of the water, owing to the pollution from the Lehigh River, becomes objectionable; and its extension would then most economically be to the Delaware Water Gap.

"It is better to build two separate aqueducts in this way than only one with double the capacity, because in the latter case the risk from accident becomes greater. New York, Boston, Washington and Paris, have each two. London has even more.

"When the above-mentioned aqueducts are built, the city of Philadelphia will be supplied with the best water obtainable in Eastern Pennsylvania."

The REACTION of a LIQUID JET;

Being a Review of § 522 and § 523 of Weisbach's „Ingenieur- und Maschinen-Mechanik, Erster Theil; Fünfte verbesserte und vervollständigte Ausgabe, Braunschweig, 1875;" With some additional matter.

By Prof. J. Burkitt Webb, Stevens Institute, Hoboken, N. J.

This article is intended principally as an explanation of an author belonging to those few who have earned the right to be regarded as authorities, and of a method which should commend itself to all those who see in the law of conservation of energy, one of the surest bases of analysis. It is to be regretted that criticisms of such authors are often made, which a thorough examination of the subjects criticised might show to be unwarranted, and in this way, the respect due to their authority may be unjustly weakened; I hope that the tendency of this article may be in the contrary direction, and that the slight corrections, which many students, no doubt, would make for themselves, may be found reasonable by all, and may tend rather to contribute to than to detract from the value of a work, the almost-completed recent review of which should make it invaluable.

Lest the discussion of so simple a thing as the reaction of a jet of water might seem unnecessary, let me say to those to whom its action is, or seems to be, clear, that while it is simple when the general results only of the phenomenon are considered, it becomes complex when the details of the action of the individual particles are discussed, and this complexity is confusing to many, especially if it appears difficult to reconcile it with a simpler view of the action of the jet. I have, therefore, added a discussion, in a simple case of the distribution of pressures in a vessel of flowing water, for the purpose of showing the agreement of the results thus derived with those obtained from a more general standpoint.

Of course the simplest way to arrive at the reaction of a fluid jet is to seek that constant force which, acting during one second, will give the velocity of the jet to the quantity of fluid which enters it per second. This force equals the product of the mass of the

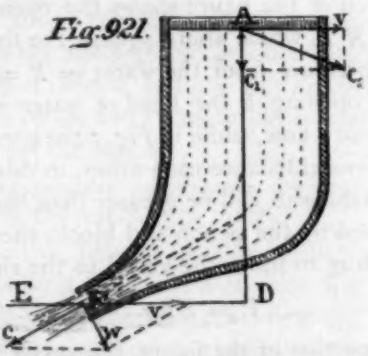
fluid into the jet velocity; the reaction of a fluid jet is therefore equal to the momentum per second of the jet.

As there is to be no discussion of the effect of friction, the otherwise useful distinction, due to Rankine, between *direct action* and *reaction*, will not be made.

I have translated the paragraphs themselves from the last German edition. In § 522 the original has been closely followed, while, in the next section, the language has been shortened somewhat, without in any way altering the sense. The equations have also been numbered, and with odd numbers alone, to allow corresponding even numbers to be used in the comments upon them.

No apology is needed for the notation of this article. To make it more systematic would evidently involve a partial change of Weisbach's notation, which, though it may be advisable in a revision of the original, would not contribute to the value of a review.

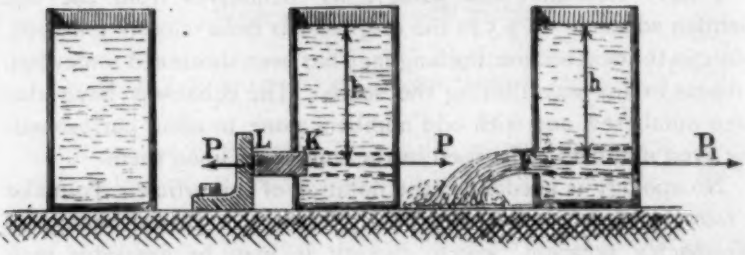
Fig. 921.



"§ 522. REACTION OF WATER.—In a vessel of still water the total pressure of the water against the vessel reduces itself to a vertical force equal to the weight of the water; when, however, the vessel *A F*, *Fig. 921*, has an opening, *F*, through which the water flows out, this pressure is changed, not only because at *F* a portion of the wall of the vessel is wanting, but also because the water, as it flows toward the orifice, reacts by virtue of its inertia, as every body does whose condition of motion is being changed. As this change may include an alteration of velocity as well as of direction of motion, so may the *reaction* of the out-flowing water arise as well from an acceleration as from a change in direction of the water flowing toward the opening."

Weisbach has already, in a previous paragraph, made a plain statement as to the reaction of a jet. The pressure of the vessel of still water downward upon its support equals the weight of the water (see *Fig. 1*), the vessel itself being supposed without weight, as

Fig. 1. *Fig. 688.*



will be done throughout this discussion. In *Fig. 688*, the effect of the removal of a portion of the wall of the vessel is illustrated; the left-hand portion of the figure shows the opening filled with a frictionless piston $K\,L$, which abuts against the fixed angle-block, L, and sustains a pressure from the water $= P = F h \gamma$, where F is the area of the opening, h the head of water in feet and γ the weight of a cubic foot: now, while in *Fig. 1* the horizontal pressures of the water on the vessel balance each other, in this figure the pressure on the right-hand wall will be greater than that on the left by the amount sustained by the piston and block, therefore the water exerts a force, tending to move the vessel to the right, equal to

$$P = F h \gamma. \qquad (1)$$

In the right-hand portion of the figure, the vessel has been moved to the right, so as to allow the water to flow out, and the statement is made that the reaction of the out-flowing water adds an equal amount and increases, therefore, the force tending to move the vessel to the right to

$$P_1 = 2\,F h \gamma; \qquad (3)$$

but proof of this is deferred, nor is the effect upon the weight of the water alluded to.

The term "inertia" were better omitted in the science of mechanics as being of no scientific value. Except so far as it is synonymous with "mass," it attaches to no definite or useful idea other than the statement that for all change of motion there must

be a cause, but, as all science is based upon the hypothesis that nothing occurs without a cause, such a statement would seem superfluous. If it be supposed that "inertia" refers to a tendency of matter to oppose itself to being moved, or having its motion changed, or to a laziness or inability to set itself in motion, then the term is not only useless but misleading, for two particles of matter left to themselves in each other's presence immediately begin to move toward each other.

The term "reaction" generally applied to these phenomena certainly does not in all cases accurately describe them. When no water flows out, the weight of each particle is transmitted unchanged to the bottom of the vessel as a downward pressure; in the other case, the particles have their motion changed and accelerated as they approach the orifice, and therefore some of them cause a greater and some a less pressure on the bottom than that due to their weights. Viewing the phenomenon in another way, an orifice in a vessel of water allows the atmospheric pressure to penetrate, so to speak, through the opening and produce an area of low pressure in the neighborhood thereof, and it is this low pressure that causes the flow toward the orifice and a diminution around it of the pressure of the water against the vessel.

"In the following manner we arrive at a knowledge of the complete reaction of the out-flowing water."

Notice the word out-flowing (ausfließenden), which the American edition lacks, because it is a distinct statement of the object of the analysis which follows. The term "out-flowing" and the stronger (perhaps too strong) terms "issuing water"—"water which issues"—used in the American version of the preceding paragraph, must not be taken as referring to the water after it has arrived at, or passed out of, the orifice, inasmuch as the effect to be considered is that of the water approaching the same.

"Let c be the velocity of the water flowing through F, c_1 the relative velocity of the water at the surface at A, G the area of this surface and h the head $A\ D$. We shall then have

$$\frac{c^2}{2g} = h + \frac{c_1^2}{2g}, \tag{5}$$

and for Q, the delivery per second

$$Q = F\,c = G\,c_1. \tag{7}$$

Suppose now that the vessel $A\ F$ is moving horizontally with a velocity v, then the absolute velocity c_2 of the entering water will be

$$c_2^2 = c_1^2 + v^2, \tag{9}$$

and, putting a for the angle of depression $E\ F\ c$ of the jet-axis, we have for the absolute velocity w of the jet

$$w^2 = c^2 + v^2 - 2\ c\ v\ \cos a. \tag{11}$$

Now the energy of the water before efflux is

$$L_1 = \left(\frac{c_2^2}{2g} + h\right) Q\gamma = \left(\frac{c_1^2 + v^2}{2g} + h\right) Q\gamma, \tag{13}$$

but after efflux it is

$$L_2 = \frac{w^2}{2g}\ Q\gamma = \frac{c^2 + v^2 - 2\ c\ v\ \cos\ a}{2g} Q\gamma; \tag{15}$$

it therefore follows that the amount of energy taken from the water and given to the vessel is

$$L = L_1 - L_2 = \left(\frac{c_1^2 - c^2 + 2\ c\ v\ \cos a}{2g} + h\right) Q\gamma, \tag{17}$$

but $h = \frac{c^2}{2g} - \frac{c_1^2}{2g}$, therefore

$$L = \frac{c\ v\ \cos a}{2g}\ Q\gamma; \tag{19}$$

from this we get for the *horizontal component of the reaction*,

$$H = \frac{L}{v} = \frac{c\ \cos a}{g}\ Q\gamma. \tag{21}$$

If in this we substitute $Q = F\ c$, and then the value

$$\frac{c^2}{2g} = \frac{h}{1 - \left(\frac{F}{G}\right)^2}, \tag{23}$$

obtained from

$$\frac{c^2}{2g} = h + \frac{c_1^2}{2g} = h + \left(\frac{F}{G}\right)^2 \frac{c^2}{2g},$$

we get

$$H = \frac{c\ \cos a}{g} F\ c\ \gamma = 2\ \frac{c^2}{2g} F\gamma\ \cos\ a = 2\ F\gamma\ \cos a\ \frac{h}{1 - \left(\frac{F}{G}\right)^2}. \tag{25}$$

If F is small compared with G we get

$$H = 2\ h\ F\gamma\ \cos\ a \tag{27}$$

and if the jet be horizontal

$$H = 2\,h\,F\gamma \tag{29}$$

Therefore, *the reaction of a horizontal jet is equal to the weight of a column of water, whose base equals the cross section of the jet, and whose height* (2 h) *is double that due to the velocity.*"

In commenting upon the above analysis, it will be of advantage to emphasize the most important height in the problem (that due to the velocity of the jet) by giving it a symbol, h', of its own, so that equation (5) becomes

$$h' = \frac{c^2}{2g} = h + \frac{c_1^2}{2g} \tag{6}$$

reducing (13) and (15) to

$$L_1 = \left(\frac{v^2}{2g} + h'\right) Q\gamma \tag{14}$$

and

$$L_2 = \left(\frac{v^2 - 2\,c\,v\cos a}{2g} + h'\right) Q\gamma, \tag{16}$$

so that equation (19) comes at once by substraction. Equation (23) may now be written

$$h' = \frac{c^2}{2g} = \frac{h}{1 - \left(\frac{F}{G}\right)^2} \tag{24}$$

and regarded solely as an equation showing the relation between h' and h, not to be used, however, in producing (25), which is to be written

$$H = \frac{c\cos a}{g} F c\gamma = 2\frac{c^2}{2g} F\gamma\cos a = 2\,F h'\gamma\cos a. \tag{26}$$

Putting $a = o$, this reduces to

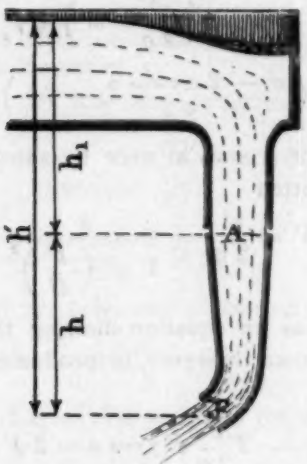
$$H = 2\,F h'\gamma, \tag{30}$$

Equations (26) and (30) are identical with (27) and (29) when h becomes $= h'$, *i. e.*, when $G = \infty$, but they have been obtained without any suppositions restricting F and G and are therefore general. In the final italicized statement of the proposition "$(2\,h)$" must also be written $(2h')$, as will be seen later; in fact, the introduction of h', while advantageous in simplifying the analysis, has for its main object the removal of a serious ambiguity in the use of h.

This analysis commences with a statement of the relations

between the water and the vessel, both c and c_1 being, of course, velocities of the former with regard to the latter. Equation (5) expresses the conservation of energy on the supposition that none is lost in heat; or, otherwise, it states that the "total head," with reference to the horizontal plane $E\ D$, is the same at A and F. Equation (7) is the "equation of continuity," expressing the fact that as fast as the water flows out at F, with velocity c, the same quantity flows in through the cross section G at A, with velocity c_1. The "surface at A" must not be taken literally to mean that there is an upper or "free" surface to the fluid, for in that case there would be no means of keeping up the supply and h, c, and c_1

Fig. 2.



would gradually decrease as the water ran out. All the equations, however, suppose that there is a "steady flow," and therefore the surface at A is simply a cross section of the stream, having, however, the peculiarity that at this section the pressure $= p_1 = 14{\cdot}7$ pounds per square inch = atmospheric pressure, the same as it would be on a free surface. To maintain the pressure p_1 constant the water must be supplied at A at exactly the speed with which it flows away from A and, to make sure that it is, the pipe should be cut apart at A to allow the atmosphere to have access to the stream; water coming out of the crack will indicate that it is being supplied faster than it can get away with the velocity c_1,

which is dependent upon h, G and F, as shown by equations (5) and (7); air going in will show the reverse. One means of furnishing the right amount is indicated in *Fig. 2*, where h_1 is the height due to the velocity c_1. The joint at A also makes the vessel $A\ F$, an independent piece, so that by supporting it suitably by scales, the effect of the flowing water could be experimented upon.

The analysis now assumes that any uniform motion given to the vessel cannot affect the flow of the water; of course the supply arrangements must go along at the same velocity, the cut in the pipe, however, serves to keep the vessel free from all other parts. If the vessel is still with reference to the earth the relations between the kinetic energies with reference to the same are given by eq. (5), or (6), when, however, it moves the new velocities, eqs. (9) and (11), and from them the new energies, eqs. (13) and (15), or (14) and (16), must be calculated with respect to the earth.

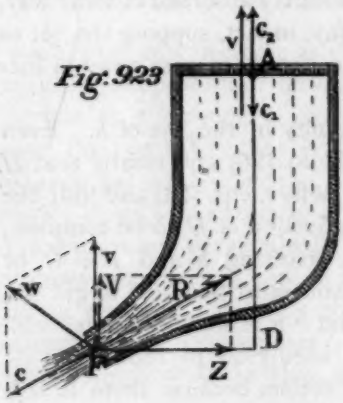
The argument now is that, as to the motion of the water with reference to the vessel, eq. (5), or (6), shows that no energy can disappear by the simple action of the head h in increasing c_1 to c; if, however, we consider the energy of the water with reference to the earth, the energy with which it enters the vessel, increased by its fall through the height h, should equal the energy of the issuing water, provided no force exists tending to move the vessel horizontally. If, however, there be a force H tending to increase the velocity v, then Hv, the work done by this force per second, must equal the energy which disappears per second from the water in flowing through the vessel. This energy, spoken of as being "given to the vessel," must be immediately absorbed in some way, so as to cause no change in v; we may, in fact, suppose the jet to be propelling a car or ship, and the energy to pass away in frictional and other resistances.

We will now consider the ambiguity in the use of h. Even without the specific introduction of h' eq. (25) says clearly that H depends upon c and a and, as shown before, eqs. (26) and (30), the demonstration requires only the introduction of h' to be complete, but if the last expression in (25), involving h and $F \div G$, be accepted as the value of H, some means must be taken to get rid of $F \div G$. Now, the supposition that "F is small compared with G," is somewhat misleading; it will hardly do to reduce the jet to nothing in attempting to find its action, because there is still another F in the expression, which is to be retained, and further,

because there is no necessity for the reduction. What we must suppose intended is to place $G = \infty$; that is, we may suppose the cross section of the supply to be enlarged until the velocity c_1 becomes 0, then any effect due to this velocity will have disappeared and we shall arrive at the action of the out-flowing water alone, without having altered the size of the jet. In this way the analysis as it stands is strictly correct, providing that the italicized paragraph be understood to be limited to the case $G = \infty$ (or, approximately, G very large), under which supposition it has been arrived at and for which only $h = h'$. This paragraph, as before stated, becomes true for all values of F and G by the substitution of $(2h')$ for $(2h)$, but in this case it follows directly from (25), or (26), any supposition regarding F or G being out of place. The supposition $G = \infty$ is also to be avoided because of the ∞ horizontal velocity required to bring distant water to the neighborhood of F, which for some minds would weaken the argument. The fact is that eq. (25) shows plainly, in spite of the introduction into it of F and G, that the entering velocity has nothing to do with H; the final equation and statement should, therefore, be based directly upon equation (25), or (26).

In the American edition the fraction $F \div G$ is not introduced and consequently no condition is imposed as to its value, but a result is obtained by substituting h for $c^2 \div 2g$, the error of which will be again referred to in considering the next section.

"§ 523. Suppose now, *Fig. 923*, the vessel's velocity v to be vertically upward, the absolute velocities of the in- and out-flowing water will be respectively



$$c_2 = v - c_1 \qquad (31)$$

and

$$w^2 = c^2 + v^2 + 2\,c\,v\,\cos(90^0 + a)$$
$$= c^2 + v^2 - 2\,c\,v\,\sin a; \qquad (33)$$

hence the total energies of the water per second are respectively

$$L_1 = \left(\frac{(v - c_1)^2}{2g} + h\right) Q\,\gamma \qquad (35)$$

and

$$L_2 = \frac{c^2 + v^2 - 2\,c\,v\,\sin a}{2g}\,Q\,\gamma; \qquad (37)$$

consequently the work transferred to the vessel is

$$L = L_1 - L_2 = \left(\frac{c_1^2 - 2\,v\,c_1 - c^2 + 2\,c\,v\,\sin a}{2\,g} + h\right) Q\,\gamma, \quad (39)$$

which by (5) reduces to

$$L = \frac{(c\,\sin a - c_1)\,v}{g}\,Q\,\gamma, \quad (41)$$

and the corresponding vertical force is

$$V = \frac{L}{v} = \frac{c\,\sin a - c_1}{g}\,Q\,\gamma = \left(\sin a - \frac{F}{G}\right)\frac{c}{g}\,Q\,\gamma$$

$$= \left(\sin a - \frac{F}{G}\right)\frac{c^2}{g}\,F\,\gamma = \left(\sin a - \frac{F}{G}\right) 2\,h\,F\,\gamma. \quad (43)$$

If F is small compared with G, we have $F \div G = 0$ and therefore the *vertical component of the reaction*

$$V = 2\,h\,F\,\gamma\,\sin a \quad (45)$$

which, combined with eq. (27), gives for the *complete reaction of the water*,

$$R = \sqrt{V^2 + H^2} = 2\,h\,F\,\gamma \quad (47)$$

in a direction exactly opposite to that of the jet."

It should be noted here that this is the end of the analysis proper, which consists in determining the horizontal and vertical reactions and then combining them into the resultant reaction; what follows consists of applications to particular values of a and $F \div G$. It should also be remarked that the supposition already made as regards the latter, which, as shown, is essentially a supposition as regards G alone ($G = \infty$) is simply a device for eliminating the effect of the entering water, while the suppositions which follow are to show the application of the result to special cases.

The introduction of h' gives in place of (35) and (37)

$$L_1 = \left(\frac{v^2 - 2\,c_1\,v}{2\,g} + h'\right) Q\,\gamma, \quad (36)$$

and

$$L_2 = \left(\frac{v^2 - 2\,c\,v\,\sin a}{2\,g} + h'\right) Q\,\gamma; \quad (38)$$

from which we get at once (41).

The last expression in (43) is evidently in error, inasmuch as h

has replaced $\frac{c^2}{g}$ in the preceding value, to which it is not equal until $F \div G$ has been made equal to zero; but this is probably accidental and has no effect on what follows. This last value for the vertical force should therefore be

$$V = 2\,F\gamma\left(\sin\alpha - \frac{F}{G}\right)\frac{h}{1-\left(\frac{F}{G}\right)^2}$$

which corresponds in form with the final value of (25).

By the introduction of h' (43) takes the form

$$V = \frac{c\sin\alpha - c_1}{g}\,Q\gamma = \left(\sin\alpha - \frac{F}{G}\right)\frac{c^2}{g}\,F\gamma = 2\,F\,h'\gamma\left(\sin\alpha - \frac{F}{G}\right) \tag{44}$$

Having pointed out the distinction between h and h' and the fact that the reaction of the jet depends upon the latter we will now call attention to a similar distinction to be made as regards the different Vs. It will be noticed that V is not called the *vertical reaction* previous to eq. (45), and it becomes so then only because $G = \infty$. V in (43) and (44) includes the vertical reaction of the entering water; the weight of the water in the vessel is not included and will be referred to later. Writing again the values for H and V, from (21) and (43) it will be evident that they contain a full solution of the problem.

Evidently they may be thus written;

$$H = Q\frac{\gamma}{g}.\,c\cos\alpha \tag{42}$$

and

$$V = Q\frac{\gamma}{g}.\,(c\sin\alpha - c_1), \tag{44}$$

where $Q\,\frac{\gamma}{g} =$ mass of water flowing per second, $c\cos\alpha =$ the horizontal component of the velocity of the jet, and $c\sin\alpha - c_1 =$ the vertical component of the same less the vertical component (= whole velocity) of the entering water. We have, therefore, $H =$ horizontal momentum of jet, $V =$ vertical momentum of out-flowing jet plus the vertical momentum of in-flowing jet, which latter is negative.

The fact that V is the sum of the actions of both out- and in-flowing water is evident also from the fact that the value of V consists of the sum of two terms, one of which depends entirely upon the entering velocity and the other upon the vertical component of the velocity of the jet. If $\alpha = 0$ V depends entirely upon c_1 and the weight of the vessel of water will be apparently increased by the momentum of the in-flowing jet only, but for a downward jet ($\alpha = 90°$) V depends on the difference of velocities, *i. e.*, upon the increase of velocity as the water passes through the vessel. Comparing also the two equations we see that the action of each jet is in a line with its axis, the in-flowing jet having no horizontal reaction and the out-flowing one having the regular form for its horizontal and vertical reactions that any force at an angle α would have. We are therefore justified in separating these parts of V and writing

$$V' = Q \frac{\gamma}{g} c \sin \alpha \tag{19}$$

and

$$R = \sqrt{H^2 + V'^2} = Q \frac{\gamma}{g} c$$

where H, V' and R are the horizontal, vertical and oblique (or total) reactions of the out-flowing jet only, and equal its horizontal, vertical and total momenta, which was the problem originally proposed.

With the introduction of h', these values become

$$H = 2 F h' \gamma \cos \alpha \tag{26}$$
$$V' = 2 F h' \gamma \sin \alpha \tag{46}$$
$$R = 2 F h' \gamma \tag{48}$$

In *Fig. 923*, the horizontal reaction is marked Z; the vertical force drawn, and marked V, is evidently V' because the resultant of it and Z is drawn in the axis of the jet.

In the American edition the following errors are to be noted in the above analysis: In (35) c appears instead of c_1, in (37) γ is omitted and in (39) the minus sign of $2 v c_1$ is lacking.

We come now to that part of the analysis devoted to the consideration of special cases.

"If $F = G$, that is, if the water flows through a uniform pipe, $F + G = 1$ and therefore,

$$V = (\sin \alpha - 1) 2 F h \gamma = -(1 - \sin \alpha) 2 F h \gamma \tag{49}$$

and V does not act upward, but downward.

For $a = -90°$, that is, when the pipe has a semi-circular bend, we have

$$H = 0 \text{ and } V = R = -\left(1 + \frac{F}{G}\right) 2 F h \gamma, \tag{51}$$

which last value becomes, for $F = G$,

$$V = R = -4 F h \gamma \tag{53}$$

Fig. 924.

If $a = +90°$, a condition represented by *Fig. 924*, we have

$$H = 0 \text{ and } V = R = \left(1 - \frac{F}{G}\right) 2 F h \gamma \tag{55}$$

and, consequently, for $F \div G = 0$,

$$V = R = 2 F h \gamma. \tag{57}$$

The total weight of the water in the vessel will be diminished by this amount when the water is allowed to flow."

We have here three cases considered: the general case $F = G$; the case $a = -90°$, with the sub-case $F = G$; and the case $a = +90°$, with the sub-case, $F \div G = 0$. It should be noticed that when $F = G$ it is not to the action of the out-flowing jet alone that the equations refer, for they give the values of V, and not of V', and to this extent they are an extension of, or a digression from, the original problem. The analysis is, however, the more elegant, inasmuch as it covers the action of both the out-flowing and in-flowing jets, and gives, therefore, their effect upon the weight of the water.

(*To be continued.*)

BOOK NOTICES.

DIE SCHIFFSMASCHINE, IHRE CONSTRUCTION, WIRKUNGSWEISE UND BEDIENUNG. Mit einem Atlas lithographirter Tafeln, bearbeitet von Carl Busley, kaiserlicher Marine-Ingenieur, Lehrer an der kaiserlichen Marine-Akademie und Schule in Kiel, etc. Kiel. Lipsius & Fischer. 1885.

This excellent book, which has been worked up with true German thoroughness, supplies a long-felt want among German engineers, ship-builders, naval officers, and others interested in shipping, who have been, during a long struggle to develop German naval and mercantile ship and marine engine construction, dependent on foreign (mainly French and English) literature for information. Considering how rapid has been the march of progress in the last twenty years, it is no small compliment to the author to say that his work is not only up to the latest improvement, but that he has produced a book whose equal is not to be found in any other language, either for the complete theoretical treatment of all subjects connected with steam-shipping, or for the mass of information on the latest practice, which is given by the extensive collection of drawings and their description.

The author starts from first principles, by stating the laws of matter, motion, energy, etc., briefly; then going into the mechanical theory of heat, its application to gases, and especially to steam. Combustion and combustibles are next taken up, followed by theories on performance, proportions and power of marine engines, coal consumption, economy, efficiency, proper rates of expansion, speed and efficiency of steamships, etc.

Coming now to practice, we find a description of steam boilers, giving types, material, construction, furnaces, mountings, lagging, arrangements of fire-rooms, bunkers, etc.; in fact, everything that is necessary to get the machinery ready for service. A section on management of boilers—their behavior when in service, corrosion, incrustation, etc.—winds up with a chapter on explosions, which is principally remarkable for refuting some of those extraordinary theories formerly current on that subject, and adopting a common-sense view.

The next chapter is devoted to laws and regulations governing construction and management of steam boilers in the German Empire, and also those adopted by the German Admiralty.

The next chapter treats fully of all auxiliary machinery on board modern merchant or naval vessels, such as turret-turning engines, air compressors, windlasses, steering engines, winders, electric light plant, etc.

The next chapters come to the marine engine itself, with all its interesting details, describing the different types up to the triple-expansion system, which is but briefly alluded to in a doubting manner, which the author, in the light of the latest developments, would now, perhaps, be ready to modify. A very complete description of valve gears and diagrams closes the first volume.

In the second volume, after going fully into all other details of the marine engine, pumps, injectors, etc., the equipment of engines (lubrication, instruments, lamps, etc., erection and completion, pipes, valves, etc.) is described,

weights of all parts are given, and several examples of complete specifications. The indicator and its application, management of marine machinery, repairs, trial trips, etc., all these with special application to naval vessels, are fully treated. Spare parts, tools, utensils, stoves, material for repairs, etc., come next, while finally the propulsion of vessels is extensively gone into, first with regard to laws of resistance, then the construction of propellers, including paddle wheels, screws, hydraulic wheels, etc.

Vol. iii contains, on 170 plates, about 1,200 lithographed drawings, all made to scale from working drawings, and executed in the best manner. This part of the book alone is a valuable record of the gradual progress and present practice of all details in marine engine construction and its adjuncts.

It may safely be predicted that a translation of this valuable work into English, would be readily disposed of in England and the United States.

Philadelphia, June 1, 1887. JOHN HAUG.

LEVELLING. By Prof. Ira O. Baker. Van Nostrand's Science Series.

This little work is eminently practical in the true sense of the word. It may be called a criticism, too, on levelling, since it points out the defects in particular of each process. Thus it gives to the student a correct appreciation of the barometer, the vertical circle and the spirit level. We are pleased to see a work of this character appear. Writers of more pretentious volumes labor often under the impression that a plea needs to be made for art and science. Hence they bring forward abnormally good results and remarkably close agreements as examples. Most readers, unfortunately, take them as *current* practice, and afterward become disappointed by personal experience. Thus arises much of the talk about the conflict of theory and practice. We recommend Prof. Baker's essay also for the clearness and conciseness with which it is written. It contains much serviceable matter to the student and to the practitioner, well expressed. In discussing precise spirit-levelling, the author seems to dwell upon the coast survey practice in preference to the European; but we prophecy that the labor and complication of the former will cause it to be abandoned in this country. E.

SCIENTIFIC NOTES AND COMMENTS.

ASTRONOMY AND PHYSICS.

A NEW METEOROLOGICAL OBSERVATORY.—A. Lawrence Rotch has recently published the *Results of the Meteorological Observations made at Blue Hill Meteorological Observatory, Massachusetts, in the Year 1886*, under his own direction. Great Blue Hill, on which the observatory is located, is 635 feet above the sea level, and is not only the highest land in Eastern Massachusetts (about ten miles out from Boston), but is also the highest point within ten miles of the Atlantic Coast from Maine to Florida. The observatory was erected at a cost of about $3,500, and the current yearly expenses of about

$2,500 are borne by the director. Considerable interest attaches to this new enterprise, from the fact that it is now not only probably the best equipped meteorological observatory on this Continent, but because the observations are, as they should be, made for the most part with automatically recording instruments, with occasional checking by direct eye readings. If the science of meteorology is ever successfully to refute the charge of publishing tome upon tome of disconnected and aimless observations, it must be through observatories that furnish *continuous* records of *all* the weather factors. Although the scope of Mr. Rotch's observatory is wider in this respect than that of any American institution we know of, it is hoped this is but a beginning in an attempt to automatically record and observe every known factor. It is gratifying to note the character of the equipment. It consists of a standard barometer by Hicks, compared at Kew, and one by Green; a Draper self-recording mercurial barometer and an aneroid barograph by the Richard Brothers, Paris; thermometric standards by Hicks and by Baudin, with working standards by H. J. Green, a Richard's thermograph; a Koppe hair hygrometer, wet and dry bulb thermometers; a Draper recording anemoscope, a Draper recording anemometer, a Robinson anemometer, with the Gibbon self-register; a Rotch wind-pressure gauge; Rotch self-recording rain and snow gauges; the Cambell-Stokes sunshine recorder, a Jordan sunshine recorder; a cloud mirror for the measurement of the azimuth and altitude of clouds, and other instruments. The results are printed in neat tabular form, and are accompanied by tracings from the self-recording instruments, to illustrate certain meteorological phenomena. M. B. S.

On the Scintillation of Stars.—K. Exner (*Astron. Nach.*, **116**, 106), in view of the present problem of determining places where large instruments would suffer least from atmospheric disturbances, draws attention to the necessity of there studying the scintillation of stars. It is well known that the larger the aperture of the instrument, the less unsteadiness is noticed in fixed stars, and this, as Newton long since remarked, arises from the fact that the image is an integration of numberless rapidly vibrating independent rays, and, is consequently, larger and as a whole relatively quiescent. To show the independent movements, there was placed before the twelve-inch refractor of the Vienna Observatory a cap with three small openings lying in a straight line. On directing to *Sirius* and pushing in the ocular, the three round images of the star appeared in continual relative motion. Observation, by means of an instrument of large aperture, therefore shows the stars as quiescent discs, or as circles of dispersion due to scintillation (*Scintillationszerstreuungskreise*) whose radii equal the amplitude of vibration, which the star would show with reduced aperture. Authorities are cited to show the numerical value of this amplitude, Holetschek finding it 5″ to 7″·5, Struve, 4″; Carlini oscillations of 10″ to 12″, while Dr. Exner, by scintillometer observations, determined the amplitude at 6″. Scintillation is far greater by day than by night; Montigny measuring its amplitude by day as great as 25″.

The advantage of scintillometer observations is considered by the author